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AND

THE ARTS.

VOL. XXI.

Illustrated with Engravings.

BY WILLIAM NICHOLSON.

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PREFACE.

THE Authors of Original Papers and Communications in the present Volume are, John Gough, Esq.; Dr. Thomas Stewart Traill; Opsimath; A. Dilethante; Mr. William Cooke; Mr. William Skrimshire, Jun.; Mr. Robert Bancks; P. Barlow, Esq.; A Correspondent; Dr. Beddoes; J. B.; John Bostock, M. D.; W. Saint, Esq,; Mr. A. Combes; J. A. De Luc, Esq.; William Walker, Esq.; W. Moore, Esq.; James Woodhouse; Mr. B. Cook; Mr. J. Acton; S. Vince.

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And of British Memoirs abridged or extracted, Thomas Andrew Knight, Esq.; Lord Ribblesdale; Thomas Thomson, M. D. F. R. S.; Mr. William Hardy; Mr. Henry Ward; Mr. Martin Furniss; Abraham Parsons, Esq.; Mrs. Hooker; Mr. William Murdoch; Everard Home, Esq. F. R. S.; Mr. Gilbert Gilpin; Mr. Christopher Wilson; J. Witley Boswell, Esq.; Major Spencer Cochrane; William Hyde Wollaston, M. D. Sec. R. S.; Mr. George Smart; Mr. Joseph Davis; Mr. S. Mendham; Mr. Edward Massey; Edmund Turrell; Robert Buchanan; Mr Joseph Collier; Mr. William Shipley; Humphry Davy, Esq. Sec. R. S. M. R. I. A.

The Engravings consist of 1. Dr. Traill's Mercurial exhausting Machine; 2. Problem by J. Gough, Esq.; 3. Mr. Hardy's Correction of Vibration in Time Keepers; 4. Mr. Henry Ward's Compensation Pendulum; 5. Mr. Furniss's Air-tight Door Hinge; 6. Mr. C. Gilpin's Machine for raising Coals; 7. Mr. C. Wilson's secure Sailing or Life Boat; 8. Mr. J. Boswell's improved Capstan; 9. Mr Mendham's Escapement; 10. Mr. Davis's Chimney Brush; 11. Mr. Davis's Pannels for Security; 12. M. Tonnelier on the Meïonite; 13. Mr. Massey's Patent Log; 14. Mr. Massey's Sounding Machine; 15. Diagrams illustrating the Problem respecting the Radius of Curvature; 16. Instruments for the Construction of improved Chemical Muffles; 17. Mr. Collier's Ship's Stove; 18. Mr. Shipley's Floating Light; 19. Mr. Acton's Improvement in the Still; 20. Crystals of Carbonate of Lime.

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SUPPLEMENT TO VOL. XXI.

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

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THE ARTS.

No. 49
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SEPTEMBER, 1808.

ARTICLE I.

A Mathematical Problem: by JOHN GOUGH, *Esq.*

To Mr. NICHOLSON.

SIR, *Middleshaw, August* 5, 1808.

THE insertion of the following problem, with the investigation of it, in your valuable Journal, will oblige

Yours, &c.
JOHN GOUGH.

The problem proposed.

Problem. To a given arc of a circle a, let it be required to add another z, making the sum of the two arcs equal to the tangent of the latter, t, viz. $a+z=t$.

Contains no thing absurd.

(A) We may show in the following manner, that the problem contains nothing absurd in it; but that, on the contrary, there is a value of z to each value of a, which would fulfil the conditions of the question, were we but able to rectify the circle.

This assertion demonstrated.

(B) Let A B C, Pl. I, fig. 6, be the given arc, consisting of n quadrants, n being any positive number, whole or fractional; to this add the quadrant C P D, in which take

 the

...rc C E; and through E, from the centre O, ...eeting the tangent C T, in F; put C O $=r$, ...C E$=x$, corresponding tangent C F$=y$; then ...ture of the circle, as $s^2:r^2::\dot{y}:\dot{x}$; but s^2 is ... than r^2, therefore $\dot{y}$ is greater than $\dot{x}$, consequently y increases faster than x: moreover, when $y=0$, $a+x=a+0=a$, in which case $a+x$ is infinitely greater than y, but y increases faster than x, and exceeds it infinitely, when $x=$ the quadrant C P D; consequently, by prime and ultimate ratios, there is a point P betwixt C and D, which cuts off an arc C P, or z, the tangent of which, C T, or $t=$ the arc A C P, i. e. $a+z=t$.

The geometrical form of the problem.

(C) When A C P $=$ C T, the sector A O P $=$ triangle C O T; from each take the common sector C O P, and the sector A O C $=$ the space C P T; hence the problem, treated geometrically, assumes this form; to find a point T in the tangent C F, produced if necessary, from which if T O be drawn to the centre, it shall give the space C P T $=$ the given sector A O C, for this construction will evidently make the tangent C T $=$ the arc A C P.

When $a=0$, $z=0$, or is infinitely small.

(D) If the arc A B C $=0$, the sector A O C $=0$: therefore the space C P T$=0$, by (C); hence the arc C E P$=0$, i. e. when $a=0$, z is evanescent; consequently, the problem is impossible, unless a be a finite magnitude.

n not restricted to odd numbers.

(E) It appears from (B) and (D), that z is a real, not an imaginary arc, provided a be a finite magnitude, which may be expressed by n Q, Q being a quadrant, and n a positive number, either whole or fractional. This conclusion however is rejected by a celebrated mathematician, who intimates, that n is always an odd number; the passage containing his opinion is here quoted.

"Invenire omnes arcus, qui tangentibus suis sint æquales.

"*Solutio.* Primus arcus, hac proprietate præditus, est infinite parvus. Tum in secundo quadrante, quia hic tangentes sunt negativæ, datur nullus istiusmodi arcus; in tertio vero quadrante dabitur unus 270° aliquanto minor; porro dabuntur ejusmodi arcus in quinto, septimo, &c."

The reason assigned for n being an odd number in this quotation is derived from the supposition, that all the tangents

gents are negative in even quadrants. To examine this reason on its own principles, let us suppose the given arc a to begin at B in the figure, not at A, as in article (B); then $a =$ arc B C $=$ a quadrant, and $n = 1$, this makes C E D the second or an even quadrant: through B and C draw the tangents B R, T C R, and the angle T R B is manifestly right; that is, C T is perpendicular to B R, but B R is a positive tangent, because B C is an odd quadrant, and T C has been shown to be perpendicular to B R, which invalidates the reason why n should be odd, because the relations of perpendicular lines are not the relations of $+$ and $-$, or positive and negative quantities.

Mutual relations of a & z.

(F) Certain mutual relations of a and z may be investigated in the following manner. Suppose these arcs to vary, so as to preserve the equation $a + z = t$, and we have $\dot a + \dot z = \dot t$; hence $\dot a = \dot t - \dot z$, but $\dot t = \frac{s^2 \dot z}{r^2} = (t^2 + r^2) \times \frac{\dot z}{r^2}$, therefore $\dot a = \frac{t^2 \dot z}{r^2}$. Now when z is less than $\frac{Q}{2}$, t^2 is less than r^2, and $\dot a$ is less than $\dot z$; but a and z begin together by (D), or the present article; therefore, when r is a small arc, it is greater than a, and $z - a$ is the greatest, when the angle C O P $= 45°$; on the contrary, when $z = Q$ it is finite, and a maximum, and a is also a maximum, but infinite; hence we have the following equations, $a = (n-1) \times z$, $t = a + z = n z$, where n is any number, whole or fractional, greater than unity.

z found by approximation, a being given.

(G) These things being premised, we may find z by approximation to any given value of a, thus: put the given arc $a + Q$, or A C D $= g$, $r = 1$, arc P D $= v$, its tangent or cotangent of $z = q$; then by the problem $g - v = t$, but by trigonometry $t \times q = r^2 = 1$, and $q = v + \frac{v^3}{3} + \frac{2v^5}{15} + \frac{17v^7}{315} + \frac{62v^9}{2835} + \frac{1382v^{11}}{155925} + \frac{21844v^{13}}{6081075} + \frac{929569v^{15}}{638512875} +$ &c. Now put $\frac{1}{3}$, $\frac{2}{15}$, $\frac{17}{315}$, $\frac{62}{2835}$, &c. $= b, c, d, e$, &c. respectively; and we have $t q = g v - v^2 + g b v^3 - b v^4 + g c v^5 - c v^6 + g d v^7 - d v^8 + g e v^9 - e v^{10}$, &c. $= 1$; and by reverting the series, as in Emerson's Algebra, page 171, Problem LXII) we get $v =$

$\frac{1}{g}$

$\frac{1}{g}+\frac{1}{g^3}+\frac{2-g^2b}{g^5}+\frac{5-4g^2b}{g^7}+\frac{14-15g^2b+3g^4b^2-g^4c}{g^9}+\frac{42-56g^2b+28g^3b-6g^4c-7g^4b}{g^{11}}$, &c.; but v is the complement of z; therefore $z=Q-v=1{\cdot}570796-v$ nearly.

Example to the last paragraph.

(H) For example, let $a=9\times Q$; then $g=10\times Q=15{\cdot}70796$; whence $\frac{1}{g}=\cdot063662$; $\frac{1}{g^3}=\cdot000266$; $\frac{2-g^2b}{g^5}=\cdot000089$. Stopping here we get $v=\frac{1}{g}+\frac{1}{g^3}+\frac{2-g^2b}{g^5}=\cdot063839$; but arc $CP=Q-v=1{\cdot}506957$: dividing this by $\cdot017453$ the length of a degree to the radius unity, we get 86° 20′ 37″ for the angle subtended by the arc C P, or z.

v in the last article is too great.

(K) Since $t=g-v=15{\cdot}644121$; and $q=v+\frac{v^3}{3}+\frac{2v^5}{15}$ &c. $=\cdot063992$; we have $t\times q=1{\cdot}001098$, &c.; but $tq=1$ universally, which shows, that $g-v$ exceeds the true value of t, or $\frac{1}{q}=15{\cdot}62386$, &c.; therefore v is a little too great, which makes z too little: but it is to be remembered, that the true place of P has been nearly found in an even quadrant.

z found when $t=nz_{\text{ш}}$.

(L) It appears from (F), that we may put $t=nz$; moreover, we have (by trigonometry) $qt=r^2=1$, hence $qz=\frac{1}{n}$; put $z=Q-v$, Q being a quadrant or $1{\cdot}570796$; also, put $q=v+bv^3+cv^5+dv^7$, &c., as in (G), and we have $Qv-v^2+Qbv^3-bv^4+Qcv^5-cv^6$ &c. $=\frac{1}{n}$; and by reversing the series, we get $v=\frac{1}{Qn}+\frac{1}{Q^3n^2}+\frac{2-Q^2b}{Q^5n^3}+\frac{5-4Q^2b}{Q^7n^4}$, &c., where n is always greater than unity; but when v is known, z is given $=Q-v$.

Example to the last paragraph.

(M) Put $n=100$, then $\frac{1}{Qn}=\cdot000636$, $\frac{1}{Q^3n^2}=\cdot000028$; stopping here, we get $v=\cdot000664$, and $z=1{\cdot}570132$, or 89° 57′ 48″.

(N) When

(N) When a is but small, the series given in (G) converges but slowly, in which case the following approximation may be used. Since $a+z=t$, $z=t-a$, but $z=t-\frac{t^3}{3}+\frac{t^5}{5}-\frac{t^7}{7}+\frac{t^9}{9}$ &c.; hence $\frac{t^3}{3}-\frac{t^5}{5}+\frac{t^7}{7}-\frac{t^9}{9}$ &c. $=a$; put $3\times a=p$, and we have, by Emerson's Algebra, page 76, $t=p^{\frac{1}{3}}+\frac{p}{5}+\frac{3p^{\frac{5}{3}}}{175}-\frac{59p^{\frac{7}{3}}}{525}$, &c.

(P) For example, let $a=\cdot009$, then $p=\cdot027$, and $p^{\frac{1}{3}}=\cdot3$, and substituting the successive powers of $\cdot3$, or $p^{\frac{1}{3}}$, we get $t=\cdot3054157$, and $z=t-a=\cdot2964157$.

(Q) When n, in (L), consists of a unit and a small fraction, we may also approximate to the value of t, by help of the two values of z, viz. $\frac{t}{n}$, and $t-\frac{t^3}{3}+\frac{t^5}{5}-\frac{t^7}{7}$, &c.; from which we get $\frac{t^2}{3}-\frac{t^4}{5}+\frac{t^6}{7}-\frac{t^8}{9}$, &c. $=n-\frac{1}{n}$: call the small fraction, $\frac{n-1}{n}$, w; and we have by reversion, $t^2=3\times w+5\cdot4\times w^2+7\cdot86857139\times w^3+10\cdot33714521\times w^4+12\cdot8037915\times w^5$, &c.

II.

On the Inconvertibility of Bark into Alburnum. By THOMAS ANDREW KNIGHT, *Esq. F. R. S. In a Letter to Sir* JOSEPH BANKS, *K. B. P. R. S.**

My dear SIR,

IN a letter which I had the honour to address to you in the end of last year†, I endeavoured to prove, that the matter, which composes the bark of trees, previously exists in the cells both of their bark and alburnum in a fluid state; and that this fluid, even when extravasated, is capa- ble

Matter that composes the bark of trees fluid in the cells of both bark & alburnum.

* Phil. Trans. for 1808, P. I, p. 103.

† Phil. Trans. 1807; or Journal, vol. XIX, p. 241.

ble of changing into a pulpous and cellular, and ultimately a vascular substance; the direction taken by the vessels being apparently dependent on the course, which the descending fluid sap is made to take*. The object of the present Memoir is to prove, that the bark thus formed always remains in the state of bark, and that no part of it is ever transmuted into alburnum, as many very eminent naturalists have believed.

Always remains bark.

Experiments on the apple and crab.

Having procured, by grafting, several trees of a variety of the apple and crab tree, the woods of which were distinguishable from each other by their colour, I took off, early in the spring, portions of bark of equal length, from branches of equal size, and I transposed these pieces of bark, enclosing a part of the stem of the apple tree with a covering of the bark of the crab tree, which extended quite round it, and applying the bark of the apple tree to the stem of the crab tree in the same manner. Bandages were then applied to keep the transposed bark and the alburnum in contact with each other; and the air was excluded by a plaster composed of bees wax and turpentine, and with a covering of tempered clay.

Their bark mutually inosculated.

Interior surfaces different.

The interior surface of the bark of the crab tree presented numerous sinuosities, which corresponded with similar inequalities on the surface of the alburnum, occasioned by the former existence of many lateral branches. The interior surface of the bark of the apple tree, as well as the external surface of the alburnum, was, on the contrary, perfectly smooth and even. A vital union soon took place between the transposed pieces of bark, and the alburnum and bark of the trees to which they were applied; and in the autumn it appeared evident, that a layer of alburnum

Union took place.

A layer of al-

Extravasated animal fluids become vascular.

* I had observed this circumstance in many successive seasons; but I was not by any means prepared to believe, that such an arrangement could take place in the coagulum afforded by an extravasated fluid; and I am indebted to Mr. Carlisle for having pointed out to me many circumstances in the motion and powers of the blood of animals, which induced me to give credit to the accuracy of my observations; and to that gentleman and to Mr. Home I have also subsequently to acknowledge many obligations.

had

had been, in every instance, formed beneath the transposed pieces of bark, which were then taken off.

burnum formed beneath.

Examining the organization of the alburnum, which had been generated beneath the transposed pieces of bark of the crab tree, and which had formed a perfect union with the alburnum of the apple tree, I could not discover any traces of the sinuosities I had noticed; nor was the uneven surface of the alburnum of the crab tree more changed by the smooth transposed bark of the apple tree. The newly generated alburnum, beneath the transposed bark, appeared perfectly similar to that of other parts of the stock, and the direction of the fibres and vessels did not in any degree correspond with those of the transposed bark *.

Alburnum did not adapt its surface to that of the foreign bark.

Repeating this experiment, I scraped off the external surface of the alburnum in several spaces, about three lines in diameter, and in these spaces no union took place between the transposed bark and the alburnum of the stock, nor was there any alburnum deposited in the abraded spaces; but the newly generated cortical and alburnous layers took a circular, and rather elliptical, course round those spaces, and appeared to have been generated by a descending fluid, which had divided into two currents when it came into contact with the spaces from which the surface had been scraped off, and to have united again immediately beneath them.

Surface of the alburnum scraped.

In each of these experiments, a new cortical and alburnous layer was evidently generated; and apparently by the same means that similar substances were generated beneath a plaster composed of bees wax and turpentine, in former experiments †; and the only obvious difference in the result appears to be, that the transposed and newly generated bark formed a vital union with each other: and it is sufficiently

New cortical & alburnous layer formed.

* Duhamel having taken off, and immediately replaced, similar pieces of the bark of young elms, subsequently found, that the alburnum, which was generated beneath such pieces of bark, had not formed any union with the alburnum of the tree beneath it. But this great naturalist did not employ ligatures of sufficient power, to bring the bark and alburnum into close contact, or the result would have been different.

Duhamel's experiment defective.

† Phil. Trans. for 1807; or Journal, vol. XIX, p. 243.

evident,

evident, that if bark of any kind was converted into alburnum, it must have been that newly generated. For it can scarcely be supposed, that the bark of a crab tree was transmuted into the alburnum of an apple tree, or that the sinuosities of the bark of the crab tree could have been obliterated, had such transmutation taken place. There is not, however, any thing in the preceding cases calculated to prove, that the newly generated bark was not converted into alburnum; and the elaborate experiments of Duhamel sufficiently evince the difficulty of producing any decisive evidence in this case; nevertheless I trust, that I shall be able to adduce such facts as, in the aggregate, will be found nearly conclusive.

Young shoots of oak. No transmutation of bark into alburnum.

Examining almost every day, during the spring and summer, the progressive formation of alburnum in the young shoots of an oak coppice, which had been felled two years preceding, I was wholly unable to discover any thing like the transmutation of bark into alburnum. The commencement of the alburnous layers in the oak *(quercus robur)* is distinguished by a circular row of very large tubes. These tubes are of course generated in the spring; and during their formation, I found the substance through which they passed to be soft and apparently gelatinous, and much less tenacious and consistent than the substance of the bark itself; and, therefore, if the matter which gave existence to the alburnum previously composed the bark, it must have been, during its change of character, nearly in a state of solution. But it is the transmutation of one organized substance into the other, and not the identity only of the matter of both, for which the disciples of Malpighi contend; and if the fibres and vessels of the bark really became those of the alburnum, a very great degree of similarity ought to be found in the organization of those substances. No such similarity, however, exists; and not any thing at all corresponding with the circular row of large tubes in the alburnum of the oak is discoverable in the bark of that tree. These tubes are also generated within the interior surface of the bark, which is well defined; and during their formation the vessels of the bark are distinctly visible, as different organs; and had the one been transmuted into the other, their progressive

progressive changes could not, I think, possibly have escaped my observation. Nor does the organization of the bark in other instances in any degree indicate the character of the wood, that is generated beneath it: the bark of the wych elm *(ulmus montana)* is extremely rough and fibrous; and it is often taken from branches of six or eight years old, to be used instead of cords; that of the ash *(fraxinus excelsior)* on the contrary, when taken from branches of the same age, breaks almost as readily in any one direction as in another, and scarcely presents a fibrous texture; yet the alburnum of these trees is not very dissimilar, and the one is often substituted for the other in the construction of agricultural instruments.

Barks of wych elm and ash differ essentially,

but not the alburnum, or wood.

Mirbel has endeavoured to account for the dissimilar organization of the bark, and of the wood into which he conceives it to be converted, by supposing, that the cellular substance of the bark is always springing from the alburnum, while the tree is growing; and that it carries with it part of the tubular substance *(tissu tubulaire)* of the liber, or interior bark. These parts of the interior bark, which are thus removed from contact with the alburnum, he conceives to constitute the external bark or cortex, while the interior part of the liber progressively changes into alburnum.

Mirbel's theory.

But if this theory (which I believe I have accurately stated, though I am not quite certain, that I fully comprehend its author*) were well founded, the texture of the alburnum must surely be much more intricate and interwoven than it is, and its tubes would lie less accurately parallel with each other than they do: and were the fibrous substance of the bark progressively changing into alburnum, the bark must of necessity be firmly attached to the alburnum during the spring and summer by the contiguity, and indeed identity of the vessels and fibres of both these substances. This, however, is not in any degree the case, and the bark is in those seasons very easily separated from the alburnum; to which it appears to be attached by a substance that is apparently rather gelatinous than fibrous or vascular:

Objections to this theory.

* Chap. III, Article 5, *Traité d'Anatomie et de Physiologie Végétale.*

and

and the obvious fact, that the adhesion of the cortical vessels and fibres to each other is much more strong than the adhesion of the bark to the alburnum, affords another circumstance almost as inconsistent with the theory of Malpighi as with that of Mirbel.

Objection to Malpighi.

Duhamel's experiment of wire in the alburnum.

Many of the experiments of Duhamel are, however, apparently favourable to the theory of Malpighi, respecting the conversion of bark into alburnum; and Mirbel has cited two, which he appears to think conclusive *. In the first of these Duhamel shows, that pieces of silver wire, inserted in the bark of trees, were subsequently found in their alburnum. But Duhamel himself has shown, with his usual acuteness and candour, that the evidence afforded by this experiment is extremely defective; and he declares himself to be uncertain, that the pieces of wire did not, at their first insertion, pass between the bark and the alburnum; in which case they would necessarily have been covered by every successive layer of alburnum, without any transmutation of bark into that substance†.

His experiment of a peach bud and bark inserted in a plum stock.

In the second experiment cited by Mirbel, Duhamel has shown, that when a bud of the peach tree, with a piece of bark attached to it, is inserted in a plum stock, a layer of wood perfectly similar to that of the peach tree will be found, in the succeeding winter, beneath the inserted bark. The statement of Duhamel is perfectly correct: but the experiment does not by any means prove the conversion of bark into wood; for if it be difficult to conceive (as he remarks) that an inserted piece of bark can deposit a layer of alburnum, it is at least as difficult to conceive, how the same piece of bark can be converted into a layer of alburnum of more than twice its own thickness (and the thickness of the alburnum deposited frequently exceeds that of the bark in this proportion), without any perceptible diminution of its own proper substance. The probable operation of the inserted bud, which is a well organized plant, at the period when it becomes capable of being transposed

* Chap. III, Article 5.

† *Physique des Arbres*, Lib IV, Ch. III.

with

with success, appears also, in this case, to have been over-looked; for I found, that when I destroyed the buds in the succeeding winter, and left the bark* which belonged to them uninjured, this bark no longer possessed the power to generate alburnum. It nevertheless continued to live, though perfectly inactive, till it became covered by the successive alburnous layers of the stock; and it was found many years afterwards enclosed in the wood. It was, however, still bark, though dry and lifeless, and did not appear to have made any progress towards conversion into wood.

In the course of very numerous experiments, which were made to ascertain the manner in which vessels are formed in the reproduced bark*, many circumstances came under my observation, which I could adduce in support of my opinion, that bark is never transmuted into alburnum; but I do not think it necessary to trouble you with an account of them; for though much deference is certainly due to the opinions of those naturalists, who have adopted the opposite theory, and to the doubts of Duhamel, I am not acquainted with a single experiment, which warrants the conclusions they have drawn; and I think, that, were bark really transmuted into alburnum, its progressive changes could only have escaped the eyes of prejudiced or inattentive observers. In the course of the ensuing spring, I hope to address to you some observations respecting the manner in which the alburnum is generated.

No facts to prove, that bark is converted into alburnum.

I am, my dear Sir,

Your most obliged obedient servant,

THOMAS AND. KNIGHT.

Elton, Dec. 29, 1807.

* Phil. Trans. for 1807; or Journal, vol. XIX, p. 241.

III.

Account of a Mine of Zinc Ore, and its Application as a Paint. By the Right Hon. Lord RIBBLESDALE, *of Gisburne Park, Yorkshire**.

SIR,

Ore of zinc as a white paint.

HEREWITH I send you a specimen of white paint, which, for the sake of humanity, I trust will be found a complete substitute for that baneful article white lead.

I have used this paint for twelve years upon my house, paling, doors, &c. It is of a delicate stone tint, but becomes equal in colour by time to the best white lead, and for durability, for never blistering, and for body and adhesion infinitely superior to it.

If the specimen (which is the average of what may be ordinarily obtained, although for particular purposes it may be produced much finer), should meet with the approbation of the Society of Arts, &c., I shall at any time, with the greatest pleasure, at their request, render them all the information upon the subject in my power. I have painted four or five years ago a vessel, which is now in his Majesty's service, with this paint, and nothing can exceed the resistance which this paint makes to all the effects of salt water to decompose it.

Resists salt water.

I have the honour to be, with much respect,

Sir,

Your obedient humble servant,

RIBBLESDALE.

Additional Communication by Lord Ribblesdale, on his Ore of Zinc.

Mines.

The mines are situate at Mallam Moors, in Craven, Yorkshire, and in an extent of country of eleven or twelve thousand

* Trans. of the Soc. of Arts for 1807, p. 35. Although this ore of zinc did not appear, upon trial by various persons, fully to answer the purposes of white lead, as a basis for paint, yet it possessed sufficient merit, to induce the Society to vote their silver medal to his Lordship.

thousand acres of land belonging to his Lordship; where the mineral is found, there were formerly copper mines.

This article is found in caverns, about eight fathoms from the surface of the earth. The mineral lies in strata, along the bottoms of these caverns, which strata are from three to six feet thick, and the best coloured mineral, or whitest, lies the lowest. On the upper part of the caverns are beautiful stalactites of great hardness.

One of the caverns wherein it is found is one hundred and four yards, another forty-four, and a third eighty-four yards in length, and about fourteen yards wide.

His Lordship supposes this mineral has been sublimed by a volcano, as the stones surrounding it have been vitrified. Surrounding stones vitrified.

The mineral was first tried as a paint twelve years ago; it was previously sold, and continues to be sold to make brass at Birmingham and other places. He has sold upwards of two thousand tons, at from five to ten pounds per ton, for making brass, when mixed with copper. Tried as a paint 12 years ago. Before used for making brass.

His Lordship stated, that it has answered well for house painting externally, and the whiteness improves by time; that it will in painting cover a much larger surface than white lead paint, and he supposes it will do half as much more work; that it forms a body on the wood so hard as to resist the edge of an adze; and that it forms a strong cement betwixt two boards painted with it. Its excellencies.

That it will never peel off; that the oil paint on palings withstands the effect of moisture; and that it will mix as a basis with all other colours.

His Lordship added, that the price will not exceed that of white lead; on the contrary, he thinks, that, except in the finest preparations, upon an average it will come considerably lower. Cheap.

DEAR SIR,

PERMIT me again to assure the Society, that the body of my paint is equal to white lead, and that the ore itself is so pure, and is found in the mine so little mingled with The ore very pure.

any

any other substance, that I do not lose two pounds of the colour in a ton of the ore.

I remain, dear Sir,

Your very humble servant,

RIBBLESDALE.

IV.

On Oxalic Acid. By Thomas Thomson, *M. D. F. R. S. Ed. Communicated by* Charles Hatchett, *Esq., F. R. S.**

Oxalic acid discovered by Scheele.

Oxalic acid, from the united testimony of Ehrhart, Hermbstadt, and Westrumb, appears to have been discovered by Scheele; but it is to Bergman that we are indebted for the first account of its properties. He published his dissertation on it in 1776, and since that time very little has been added to the facts contained in his valuable treatise. Chemists have chiefly directed their attention to the formation of that acid, and much curious and important information has resulted from the experiments of Hermbstadt, Westrumb, Berthollet, Fourcroy, Vauquelin, &c. but the properties of the acid itself have been rather neglected.

Little attention paid to its properties.

My object in the following pages is not to give a complete history of the properties of oxalic acid, but merely to state the result of a set of experiments, undertaken with the view of ascertaining different particulars respecting it, which I conceived to be of importance.

I. *Water of Crystallization.*

Its water of crystallization.

Oxalic acid is usually obtained in transparent prismatic crystals more or less regular; these crystals contain a portion of water, for when moderately heated they effloresce and lose a part of their weight, which they afterwards recover when left exposed in a moist place. When cautiously heated on a sand bath they fall to powder, and lose about a third of their weight. But as the acid is itself volatile, it is not

* Philos. Trans. for 1807, p. 63.

not probable that the whole of this loss is water. To ascertain the quantity of water contained in these crystals I had recourse to the following method.

1. Seventy grains of crystallized oxalic acid were dissolved in 600 grains of water, constituting a solution which weighed 670 grains.

The acid precipitated by muriate of lime.

Fifty grains of pure carbonate of lime, in the state of calcareous spar, were dissolved in muriatic acid; this solution was evaporated to dryness, to get rid of the excess of acid, and the residue redissolved in water.

Into this muriate of lime the solution of oxalic acid was dropped by little and little as long as any precipitate fell, and the oxalate of lime thus formed was separated by the filter. Pure oxalic acid is not capable of precipitating the whole lime from solution of muriate of lime, the muriatic acid evolved being always sufficient to retain the last portions in solution.

As the muriatic acid set loose holds the last portions in solution,

It was necessary to get rid of this excess of acid; the method which appeared the least exceptionable was to saturate the muriatic acid with ammonia; accordingly when the oxalic acid ceased to occasion any farther precipitate, I cautiously added pure ammonia, till the liquid ceased to produce any effect upon vegetable blues. A copious additional precipitate of oxalate of lime was thus obtained. Oxalic acid was now added again as long as it rendered the liquid muddy. By thus alternately having recourse to the acid solution, and to ammonia, and by adding both with great caution to avoid any excess, I succeeded in separating the whole of the lime, without using any sensible excess of oxalic acid.

this saturated by ammonia.

558 grains of the acid solution were employed, a quantity which is equivalent to 58·3 grains of the crystallized acid.

2. The oxalate of lime, after being well washed and drained, and exposed for a week to the open air, at a temperature of about 60°, weighed 76 grains; but upon being left on the sand bath for some hours in a temperature between 200° and 300°, its weight was reduced to 72 grains.

3. These 72 grains of dry oxalate of lime were put into an open platinum crucible, and gradually heated to redness. By

By these means they were reduced to 49·5 grains, which proved to be carbonate of lime. The crucible was now exposed to a violent heat in a forge. Nothing remained but a quantity of pure lime weighing 27 grains.

72 dry oxalate contain 27 lime.

4. From this experiment we learn, that 72 grains of dry oxalate of lime contain 27 grains of lime. Of consequence, the oxalic acid in this compound must be 45 grains. But the weight of crystallized oxalic acid actually used was 58·3 grains, a quantity which exceeds the whole acid in the oxalate by 13·3 grains. These 13·3 grains are the amount of the water of crystallization, which either did not unite with the salt, or was driven off by the subsequent exposure to heat. Hence crystallized oxalic acid is composed of

Crystals of oxalic acid contain ·23 water.

Real acid	45·	equivalent to	77
Water........	13·3		23
	58·3		100

So that the crystals of oxalic acid contain very nearly the fourth part of their weight of water*.

II. *Alkaline and Earthy Oxalates.*

Oxalate of lime, 62·5 acid, 37·5 base.

1. The preceding experiment gives us likewise the composition of oxalate of lime. This salt, when merely dried in the open air, still retains a portion of water, which may be

This proportion confirmed by an experiment of Vauquelin.

* Vauquelin in a late dissertation on cinchona, marked with that profound skill which characterizes all the productions of this illustrious chemist, has mentioned incidentally, that the crystals of oxalic acid contain about half their weight of water. He dissolved 100 parts of cinchonate of lime in water, and precipitated by means of oxalic acid; 22 parts of crystallized oxalic acid were necessary; and the oxalate of lime formed weighed 27 grains. From this experiment he draws the conclusion which I have stated (See Ann. de Chimie, lix, 164; or our Journal, vol. XIX, p. 213). But this ingenious chemist does not seem to have been aware of the real composition of oxalate of lime. 27 grains of that salt are composed very nearly of 10 grains of lime and 17 grains of acid. But the weight of the crystals used by Vauquelin was 22; the difference, 5, is obviously the water of crystallization in 22 grains of the crystals. But if 22 grains contain 5 of water, it is obvious, that 100 contain very nearly 23. So that his experiment in reality coincides with mine.

be driven off by artificial heat. It is necessary to know, that it parts with this water with considerable difficulty,* so that a long exposure on the sand or steam bath is necessary, to get it thoroughly dry. It afterwards imbibes a little water, if it be left in a moist place. Well dried oxalate we have seen is a compound of

Dried with difficulty.

Acid	45	or per cent,	62·5	acid.
Base	27		37·5	base.
	72		100	

Though the oxalate of lime dried spontaneously can scarcely be considered as always in the same state, yet as the difference in the portion of water which it retains is not great, provided it be dried slowly in the temperature of 60°, and in a dry place, it may be worth while to state its composition. It is as follows:

Dried slowly at 60° contains 0·53 water.

Acid	45	or per cent	59·2	acid.
Base	27		35·5	base.
Water	4		5·3	water.
	76		100·0	

When rapidly dried, as by pressing it between the folds of filtering paper, it is apt to concrete into hard lumps, which retain more moisture. In this state I have sometimes seen it retain 10 per cent of water after it appeared dry.

Dried rapidly sometimes ·1 of water.

Bergman states the composition of oxalate of lime as follows:

Bergman's statement.

Acid	48
Lime	46
Water	6
	100*

His method was to dissolve a determinate quantity of calcareous spar in nitric acid, and then to precipitate the lime by oxalic acid. 100 parts of calcareous spar thus dissolved, require, according to him, 82 parts of crystallized acid to precipitate them. But there must have been some mistake

His method.

* Opusc. I, 262.

in this experiment; for, according to my trials (provided the nitric acid be carefully naturalized by ammonia as it is evolved), no less than 117 grains of oxalic acid would have been required, and at least 145 grains of oxalate of lime would have been obtained instead of the 119, which was the result of Bergman's experiment. It is obvious, that Bergman did not precipitate all the lime. He added oxalic acid till it ceased to produce any effect on the solution from the great excess of nitric acid evolved; and then took it for granted, that all the lime was separated. But had he added ammonia, he would have got an additional quantity of oxalate of lime, and the precipitation would have recommenced upon adding more oxalic acid. This explanation accounts in a satisfactory manner for the difference between Bergman's statement of the composition of oxalate of lime, and mine.

Cause of his mistake.

The preceding analysis verified.

2. Though the preceding experiment was made with care, yet as some of the most important of the following observations in some measure rest upon the analysis of oxalate of lime, I thought it worth while to verify that analysis in the following manner.

100 grains of crystallized oxalic acid were dissolved in 1000 grains of water, making a solution which weighed 1100 grains.

It is obvious, that every 100 grains of the above solution contained 9·09 grains of crystals of oxalic acid, equivalent, according to the preceeding analysis, to 7 grains of real acid.

100 grains of this solution were gradually mixed with lime water, till the liquid ceased to produce any change on vegetable blues. The oxalate of lime thus formed, being well dried, weighed 11·2 grains. Exposed to a violent heat in a platinum crucible, this salt left 4·2 grains of pure lime. Hence it was composed of

7 acid, or	per cent	62·5 acid
4·2 lime		37·5 base
11·2		100·0

Thus we have obtained exactly the same result as in the former experiment, both as far as relates to the composition of

of oxalate of lime, and likewise to the proportion of water of crystallization in crystallised oxalic acid.

The lime water necessary to saturate the acid amounted to 3186 grains. Hence, it contained only $\frac{1}{778}$th of its weight of lime. Water dissolves $\frac{1}{778}$ lime.

3. The oxalates of barytes and strontian are white, tasteless powders, which may be obtained by mixing oxalate of ammonia with the muriates of these alkaline earths. It is said, that these earths are capable of forming soluble superoxalates with this acid; but I have not tried the experiment. These oxalates, as well as oxalate of lime, are partially soluble in the strong acids. Oxalates of barytes and strontian.

4. Oxalate of magnesia is a soft white powder, bearing a considerable resemblance to oxalate of lime. It is tasteless, and not sensibly soluble in water; yet when oxalate of ammonia is mixed with sulphate of magnesia, no precipitate falls; but if the solution be heated and concentrated sufficiently, or if it be evaporated to dryness, and redissolved in water, in both cases the oxalate of magnesia separates in the state of an insoluble powder. Oxalate of magnesia.

5. Oxalate of potash readily crystallizes in flat rhomboids, commonly terminated by dihedral summits. The lateral edges of the prism are usually bevilled. The taste of this salt is cooling and bitter. At the temperature of 60° it dissolves in thrice its weight of water. When dried on the sand bath, and afterward exposed in a damp place, it absorbs a little moisture from the atmosphere. Oxalate of potash.

This salt combines with an excess of acid, and forms a superoxalate, long known by the name of *salt of sorrel*. It is very sparingly soluble in water, though more so than tartar. It occurs in commerce in beautiful 4-sided prisms attached to each other. The acid contained in this salt is very nearly double of what is contained in oxalate of potash. Suppose 100 parts of potash; if the weight of acid necessary to convert this quantity into oxalate be x, then $2\,x$ will convert it into superoxalate. Salt of sorrel.

6. Oxalate of soda readily crystallizes. Its taste is nearly the same as that of oxalate of potash. When heated, it falls to powder, and loses the whole of its water of crystallization. Soda is said to be capable of combining with an Oxalate of soda.

excess of acid, and of forming a superoxalate. I have not tried the experiment.

Oxalate of ammonia.

7. Oxalate of ammonia is the most important of all the oxalates, being very much employed by chemists to detect the presence of lime, and to separate it from solutions. It crystallizes in long trasparent prisms, rhomboidal, and terminated by dihedral summits. The lateral edges are often truncated, so as to make the prism 6 or 8-sided. Sometimes the original faces of the prism are nearly effaced.

The taste of this salt is bitter and unpleasant, somewhat like that of sal ammoniac. At the temperature of 60°, 1000 grains of water dissolve only 45 grains of this salt. Hence, 1000 grains of saturated solution of oxalate of ammonia contain only 43·2 grains of this salt. The specific gravity of this solution is 1·0186. As it may be useful to know the weight of this salt contained in solutions of different specific gravities, I have thought it worth while to construct the following table:

Specific gravity of solution of oxalate of ammonia.

Weight of oxalate of ammonia in 100 parts of the solution	Specific gravity of the solution at 60°	Weight of oxalate of ammonia in 100 parts of the solution.	Specific gravity of the solution at 60°.
4·32	1·0186	1·5	1·0075
4·	1·0179	1·	1·0054
3·5	1·0160	0·5	1·0030
3·	1·0142	0·4	1·0024
2·5	1·0120	0·3	1·0018
2·	0·0095	0·2	1·0012
		0·1	1·0006

Method of determining the combustion of the oxalates.

8. To determine the composition of these salts, I took seven different portions of a diluted oxalic acid solution, each weighing 100 grains, and containing 7 grains of real oxalic acid. To each of these portions I added respectively potash, soda, ammonia, barytes water, strontian water, and lime water, till it ceased to produce any change. The liquid was then evaporated to dryness, and the residue, after being well dried on the steam bath, was weighed. Each of these salts contained 7 grains of acid; the additional weight

weight I ascribed to the base. Hence I had the following table, which exhibits the weight of each salt obtained, and its composition deduced from that weight.

Salts.	Weight obtained	Composition Acid	Composition Base
Oxalate of Ammonia	9·4	7	2·4
——— Magnesia* . .	9·5	7	2·5
——— Soda	11·0	7	4·0
——— Lime	11·2	7	4·2
——— Potash	15·6	7	8·6
——— Strontian . .	17·6	7	10·6
——— Barytes	17·0	7	10·0

The composition of these salts reduced to 100 parts is given in the following table.

Component parts of the oxalates.

	Ox. of Ammonia	Ox. of Magnesia.	Ox of Soda	Ox. of Lime.	Ox. of Potash	Ox of Strontian.	Ox of Barytes
Acid	74·45	73·68	63·63	62·50	44·87	39·77	41·16
Base	25·53	26·32	36·37	37·50	55·13	60·23	58·84
Total	100	100	100	100	100	100	100

But for practical purposes, it is more convenient to consider the

* The oxalate of magnesia was obtained by neutralizing the oxalic acid solution with ammonia, then mixing it with sulphate of magnesia, evaporating the solution to dryness, and washing the insoluble oxalate of magnesia with a sufficient quantity of water.

the acid as a constant quantity. The following table is constructed upon this plan.

Component parts the acid being 100.

	d	Base.	Weight of Salt.
Oxalate of Ammonia	100	34·12	134·12
——— Magnesia	100	35·71	135·71
——— Soda	100	57·14	157·14
——— Lime	100	60·00	160·00
——— Potash	100	122·86	222·86
——— Strontian	100	151·51	251·51
——— Barytes	100	142·86	242·86

Oxalates retain little if any water in a moderate h a,

9. In the preceding statement, no account has been taken of the water of crystallization, which might still remain attached to the salts, notwithstanding the heat to which they were exposed. There is reason to believe, however, that in most of them this water must be so small, that it may be overlooked without any great errour. Oxalates of soda and of ammonia, I have reason to believe, lose all their water of crystallization at a moderate heat. This is the case also with oxalates of lime and barytes; and I presume, that the oxalates of strontian and magnesia are not exceptions; but oxalate of potash retains its water much more obstinately. I believe that in this salt the weight of acid and of base are nearly equal, and that when dried in the temperature of 212°, it still retains nearly 10 per cent of water; but I have not been able to establish this opinion by direct experiment.

except that of potash.

Oxalate of strontian.

The composition of oxalate of strontian in the preceding table was so different from what I expected, that I repeated the experiment; but the result was the same. This induced me to combine strontian and oxalic acid in the following manner: 100 grains of a solution containing 7 grains of real oxalic acid were neutralized by ammonia, and the oxalic acid precipitated by means of muriate of strontian. The salt obtained weighed 12·3 grains; of course it was composed of

Acid	7	or 56·9	or 100
Base	5·3	43·1	75·7
	12·3	100·0	175·7

Thus

Thus it appears, that there are two oxalates of strontian, the first obtained by saturating oxalic acid with strontian water, the second by mixing together oxalate of ammonia and muriate of strontian. It is remarkable, that the first contains just double the proportion of base contained in the second. Two species. One with double the base of the other.

III. *Decomposition of the Oxalates.*

1. When oxalic acid, in the state of crystals, is exposed to heat, it is only partially acted upon, a considerable portion escaping without alteration; but when an alkaline or earthy oxalate is heated, the acid remains fixed, till it undergoes complete decomposition. The new substances into which the acid is converted, as far as my experience goes, are always the same, what oxalate soever we employ. They are five in number; namely, *water, carbonic acid, carbonic oxide, carburetted hidrogen,* and *charcoal.* Crystallized acid in part sublimes: in oxalates decomposed by heat. Products.

2. The water is never quite pure. Though no sensible portion of oil can be perceived in it, yet it has always the peculiar smell of the water obtained during the distillation of wood; a smell which is usually ascribed to oil. It commonly shows traces of the presence of ammonia, changing vegetable blues to green, and smoking when brought near muriatic acid; but this minute portion of ammonia is probably only accidentally present. All the oxalates, which I decomposed by distillation, were obtained by double decomposition from oxalate of ammonia; and though they were washed with sufficient care, yet I think it not unlikely, that a minute portion of oxalate of ammonia might continue to adhere. Practical chemists know the extreme difficulty of removing every trace of a salt, with which another has been mixed. Water.

The carbonic acid remains partly combined with the base, which always becomes a carbonate, and partly makes its escape in the form of gas. Carbonic acid.

The carbonic oxide and carburetted hidrogen make their escape in the form of gas: the charcoal remains in the retort mixed with the base, to which it communicates a gray colour: the quantity of it depends in some measure upon the heat. If the oxalate was exposed to a very violent heat, Carbonic oxide, carburetted hidrogen, and charcoal.

no

no charcoal at all remains. Hence it probably acts upon the carbonic acid united to the base, converting it into carbonic oxide, as happens when a mixture of a carbonate and charcoal are heated.

Decomposition of oxalate of lime attentively examined.

3. I was induced to examine this decomposition with considerable attention, because I conceived, that it would furnish the means of estimating the composition of oxalic acid; and I pitched upon oxalate of lime, as the salt best adapted for the purpose I had in view. A determinate quantity of this salt was put into a small retort, and gradually heated to redness. This retort was connected with a pneumatic trough by means of a long glass tube, having a valve at its extremity, which allowed gas to issue out, but prevented any water from entering the tube. The experiment was repeated three times.

100 grs. yield 60 inches of gas.

4. A hundred grains of oxalate of lime, when thus heated, yield above sixty cubic inches of a gas, which is always a mixture of carbonic acid and inflammable air, nearly in the proportion of one part of the former to three and a half of the latter, reckoning by bulk. The specific gravity of the inflammable gas was 0.908, common air being 1·000; it burns with a blue flame, and, when mixed with oxigen, may be kindled by the electric spark. The loudness of the report depends upon the proportion of oxigen.

Mixed with oxigen & kindled by the electric spark.

The smallest quantity of oxigen, with which it can be mixed, so as to burn by the electric spark, is $\frac{1}{6}$th; the combustion is very feeble, and is attended with no perceptible report. If the residue be washed in lime water, and mixed with $\frac{1}{6}$th of its bulk of oxigen, it may be kindled a second time: this may be repeated five times, after which the residue cannot be made to burn.

The combustion becomes more violent, and the report louder, as we increase the proportion of oxigen, and both are greatest when the oxigen is double the bulk of the gas. As we increase the dose of oxigen, the combustion becomes more and more feeble; and five parts of oxigen and one of gas form the limit of combustion on this side: for a mixture of six parts of oxigen and one of the inflammable air will not burn.

In

Results differ according to the proportions of oxigen.

In these experiments the results differ materially from each other, when the proportion of oxigen used is small and when it is great. I am not able at present to account for this difference, which holds not only with respect to this gas, but every compound inflammable gas, which I have examined. This difference makes it impossible to use both extremes of the series: I make choice of that in which the proportion of oxigen is considerable, as upon the whole more satisfactory. The best proportion is one part of the gas and two parts of oxigen. The oxigen ought not to be pure, but diluted with at least the third of its bulk of azote, unless the gas be much contaminated with common air.

I have elsewhere detailed the method, which I follow in analyzing gasses of this nature *. The following table exhibits the mean of a considerable number of trials of this gas with oxigen.

Mean result of the combustion

Measures of inflammable air consumed	Measures of oxigen consumed.	Carbonic acid formed.	Diminution of bulk.
100	91	93	98

That is to say, 100 cubic inches of the gas, when burnt, combine with 91 cubic inches of oxigen; there are produced 93 inches of carbonic acid; and after the combustion these 93 inches alone remain, the rest being condensed. Hence we conclude, that the other substance produced was water.

This result corresponds almost exactly with what would have been obtained, if we had made the same experiment upon a mixture of 70 measures of carbonic oxide, and 30 measures of carburetted hidrogen, as will appear from the following table.

* See Journal, vol. XVI, p. 247.

Carbonic

	Measures of inflammable gas consumed.	Measures of oxigen consumed.	Measures of carbonic acid formed.	Diminution of bulk.
Carbonic oxide	70	31·5	63	38·5
Carburetted hidrogen	30	60·0	30	60·0
Total ..	100	91·5	93	98·5

It was a mixture of 70 carbonic oxide and 30 carburetted hidrogen.

This coincidence is so exact, that I do not hesitate to conclude, that the inflammable gas, which was the subject of experiment, was in reality a mixture of 70 parts of carbonic oxide, and 30 of carburetted hidrogen. The specific gravity indeed, which was 0·908, does not exactly agree with the specific gravity of such a mixture; for $2\frac{1}{3}$ measures of carbonic oxide, and one measure of carburetted hidrogen, ought to form a mixture of the specific gravity 0·849, provided the specific gravity of carbonic oxide be 0·956, and that of carburetted hidrogen 0·600; but this objection cannot be admitted to be of much weight, till the specific gravity of pure carburetted hidrogen is ascertained with more accuracy than has hitherto been done.

Its composition.

The results contained in the preceding table enable us to determine the composition of this inflammable air with considerable precision; for 100 cubic inches of it require 91 inches of oxigen, and form 93 cubic inches of carbonic acid. But it is known, that carbonic acid gas requires for its formation a quantity of oxigen gas equal to its own bulk: therefore to form 93 inches of it, 93 inches of oxigen gas must have been employed; but only 91 were mixed with the gas: therefore the gas itself must have furnished a quantity of oxigen, equivalent to the bulk of two cubic inches, beside all the carbon contained in 93 inches of carbonic acid.

This carbon amounts in weight to .. 12·09 grains.
Two cubic inches of oxigen weigh .. ·68

Total .... 12·77

But

But as 100 cubic inches of the gas weigh 28·15 grains, it is obvious, that, beside the 12·77 grains which it furnished to the carbonic acid, it must have contained 15·38 grains of additional matter; but as the only two products were carbonic acid and water, it is plain, that the whole of this additional matter must, by the explosion, have been converted into water. Its constituents of course must have been

13·19	oxigen
2·19	hidrogen
15·38	

Adding this to the 12·77 grains formerly obtained, we get the composition of the gas as follows:

Oxigen ······	13·87
Carbon ······	12·09
Hidrogen ····	2·19
	28·15

which, reduced to 100 parts, becomes

Constituent principles.

Oxigen ······	49·27
Carbon ······	42·95
Hidrogen ····	7·78
	100·00

Residuum.

5. The residue which remained in the retort, after the distillation was over, was a gray powder, not unlike pounded clay slate. To ascertain its constituents, it was dissolved in diluted nitric acid with the necessary precautions; the loss of weight indicated the quantity of carbonic acid. The charcoal remaining undissolved was allowed to subside, carefully washed by repeated affusions of water, and then dried in a glass or porcelain capsule. It must not be separated by the filter, for it adheres so obstinately, that it cannot be taken off the paper, nor weighed. The nitric acid solution was precipitated by carbonate of soda, and the carbonate of lime obtained was violently heated in a platinum crucible. What remained was pure lime.

89 grs. of oxa-

6. I shall now detail one of my experiments more particularly.

late of lime heated in a retort.

cularly. Eighty-nine grains of well dried oxalate of lime were exposed in a small retort to a heat gradually raised to redness; the products were the following:

	Grains.
45·6 cubic inches of gas* weighing····	14·8
Water ·································	6·4
Residue in retort ···················	62·4
	83·6
Loss ···········	5.4
Total ········	89·0

The loss is obviously owing to the gas, which filled the retort and tube when the experiment was concluded. We are warranted therefore to add it to the weight of the gaseous products obtained.

Now the gas was composed of

Carbonic acid ·· 10·5 cubic inches = 4·9 grains.
Inflammable air 35·1 ·········· = 9·9

so that one third of the weight was carbonic acid, and two thirds inflammable air. If we divide the 5·4 grains of loss in that proportion, we obtain 1·8 grain carbonic acid, and 3·6 grains of inflammable air. Adding these quantities to the weight obtained, we get for the weight of the whole gaseous product

Gaseous product.

	Grains.
Carbonic acid ····	6·7
Inflammable air ··	13·5
	20·2

The 62·4 grains of residue in the retort were composed of

Residuum.

	Grains.
Lime·············	33·4
Carbonic acid ····	26·4
Charcoal ········	2·6
	62·4

* The gas obtained measured 60 cubic inches, but 14·4 inches of these were found to be common air, which had previously filled the retort and tube; this quantity was therefore deducted.

Now

Now it is clear, that the 89 grains of oxalate of lime were composed of

Lime	33·4
Acid	55·6
	89·0

The acid was completely decomposed and resolved into the following products:

Products of 55·6 grs. of acid.

Carbonic acid	33·1
Inflammable air	13·5
Water	6·4
Charcoal	2·6
	55·6

Had the experiment been made upon 100 grains of oxalic acid instead of 55·6, it is clear, that the proportions would have been as follows.

Proportions of 100 parts.

Carbonic acid	59·53
Inflammable air	24·28
Water	11·51
Charcoal	4·68
	100·00

The most remarkable circumstance attending the decomposition of oxalic acid by heat is the great proportion of carbonic acid formed; the quantity amounts to 6 tenths of the whole weight of acid decomposed.

Constituent principles of the acid.

As the composition of all these products of oxalic acid is known with considerable accuracy, it is obvious, that they furnish us with the means of ascertaining the constituents of that acid itself.

59·53 grains of carbonic acid are composed of

	Grains.
Oxigen	42·86
Carbon	16·67
	59·53

24·28 grains of the inflammable air, according to the analysis given in a preceding part of this paper, are composed of

	Grains.
Oxigen ······	11·96
Carbon ······	10·43
Hidrogen ····	1·89
	24·28

11·51 grains of water are composed of

Oxigen ········	9·87
Hidrogen ······	1·64
	11·51

As for the charcoal, though it probably contains both oxigen and hidrogen as well as carbon, yet as the proportion of the first two ingredients is probably very small, and as we have no means of estimating them, we must at present rest satisfied with considering it as composed of pure carbon.

When these different elements are collected under their proper heads, we obtain

	Oxigen.	Carbon.	Hidrogen.
In carbonic acid ··	42·86	16·67	
Inflammable air ··	11·96	10·43	1·89
Water ··········	9·87		1·64
Charcoal ········		4·68	
	64·69	31·78	3·53

Elements.

Hence oxalic acid is composed of	oxigen ····	64·69
·························	carbon ····	31·78
·························	hidrogen ··	3·53
		100·00

Confirmed by other experiments.

7. The result of two other experiments on oxalate of lime was very nearly the same as the preceding. The following may

may be stated in round numbers as the mean of the whole. Oxalic acid is a compound of

Oxigen	....	64
Carbon	....	32
Hidrogen	......	4
		100

Mean in round numbers.

8. The only other analysis of oxalic acid, with which I am acquainted, has been given by Mr. Fourcroy, as the result of his own experiments, in conjunction with those of Vauquelin *. It is as follows:

Elements according to Fourcroy and Vauquelin.

Oxigen	....	77
Carbon	....	13
Hidrogen	......	10
		100

It gave me considerable uneasiness to observe, that my experiments led to conclusions irreconcilable with those of chemists of such eminence and consummate skill; and it was not without considerable hesitation, that I ventured to place any reliance upon them. I am persuaded, however, that some mistake has inadvertently insinuated itself into their calculations; since the carbonic acid alone, formed during the distillation of oxalate of lime, contains considerably more carbon than the whole quantity, which they assign to the oxalic acid decomposed. Mr. Fourcroy informs us, that oxalic acid is converted into carbonic acid and water, when acted upon by hot nitric acid; and this decomposition seems to have been the method employed, to ascertain the proportion of the constituents of oxalic acid; but the numbers assigned by him do not correspond with this statement. For 10 parts of hidrogen require 60 of oxigen to convert them into water, and 13 of carbon require at least 33 of oxigen. So that instead of 77 parts of oxigen, there would have been required no less than 98, to convert the hidrogen and carbon into water and carbonic acid. It is true, that the surplus of oxigen may be con-

Their calculations erroneous.

* *Système de Connois. Chem.* VII, 224. Trans. VII, 306.

ceived

ceived to be furnished by the nitric acid; but if this be admitted (and I have no doubt from experience, that the nitric acid actually does communicate oxigen), it is difficult to see how the constituents of oxalic acid could be determined by any such decomposition, unless the quantity of oxigen furnished by the nitric acid were accurately ascertained.

(To be concluded in our next.)

V.

Analysis of some Iron Ores in Burgundy and Franche-Comté, to which is added, an Examination of the Pig Iron, Bar Iron, and Scoriæ, produced from them. By Mr. VAUQUELIN *.

Ores, iron, scoriæ, and fluxes, collected for examination.

MR. Vauquelin, in the year 1805, having visited various iron works in Burgundy, collected several specimens of ores, pig iron, bar iron, scoriæ, and fluxes, for the purpose of subjecting them to chemical analysis, in order to ascertain, whether it were possible to know from a comparison of their composition, what takes place in the processes, to which iron ores and cast iron are subjected. We shall give here the leading results of this able chemist's labours, and the particulars of some of the processes he employed to obtain these results.

I. *Chemical examination of some fluor spars.*

Fluor spar employed as a flux at Drambon.

The spar employed as a flux at the mine of Drambon, in the department of Côte-d'Or, is of a yellowish white, and tolerably hard. It dissolves with effervescence in nitric acid, and leaves a yellowish residuum, amounting to about a fifth of its weight, which is composed chiefly of fine

* Journal des Mines, No. 119, p. 382. The whole of the paper, of which this an abridgment, will be found in the Memoirs of the National Institute.

sand,

sand, with a minute quantity of alumine and iron. The solution, which is colourless, gives with ammonia a light, flocculent, semitransparent, yellowish white precipitate, in which was recognized the presence of iron, a little alumine, and phosphate of lime. It likewise contained some traces of silex.

The spar of Pesme is compact, of a grayish white, and dissolves in nitric acid, leaving a residuum of about a twentieth of its weight. A little iron, alumine, and phosphate of lime, were observed in the solution. That at Pesme.

From these two analyses it appears, that the fluors analysed consist almost wholly of calcareous matter, but that of Pesme is much the most pure. They show at the same time, that the stones examined contain a small quantity of phosphate of lime, which certainly does not amount to a five hundredth part. Almost wholly calcareous, but that of Pesme most pure.

II. *Analysis of the scoriæ of the iron works at Drambon.*

Mr. Vauquelin begins with a chemical examination of these scoriæ, rather than with that of the ores and smeltings, because these scoriæ include more foreign matters in a smaller bulk. Scoriæ of Drambon.

They have a shining blackish colour, nearly resembling certain oxides of manganese. Their weight indicates, that a considerable quantity of metallic matter is left in them. Some parts exhibit blebs of different sizes, others are compact. Their fracture is crystallized, either needly or laminar. Physical characters.

Five grammes [77 grains] of scoriæ, fused twice in succession with an equal weight of caustic potash, communicated to the alkali a very deep green colour, when the mass had been washed with water. Analysed.

This green colour is known to be an unequivocal proof of the presence of manganese, and it is the best method we can employ, to discover the slightest trace of this metal in any substance. Manganese,

All the washings of the scoriæ thus treated were added together, and boiled, to separate the manganese. In proportion as this effect took place, the liquor lost its green colour, and the metal floated in it in the form of brown flocks, This separated.

flocks, which, when collected, washed, and dried, weighed 2 decig. [3 grs.] amounting to 4 per cent.

Chrome suspected.

The alkaline liquor, freed from the manganese and filtered, still retained an orange yellow colour, which led Mr. Vauquelin to suspect the presence of chrome.

Silex and alumine first separated.

For verifying this suspicion, it was necessary, in order to facilitate the operations necessary for detecting the chrome, to separate the alumine and silex, that were in the alkaline lixivium; and to avoid the presence of muriatic acid, which would have thwarted the end he proposed, Mr. Vauquelin employed very pure nitrate of ammonia, instead of the muriate. Thus he obtained 2 cent. [0·3 gr.] of a mixture of silex and alumine.

Carbonic acid expelled by nitric & boiling.

He then saturated the liquor with very pure nitric acid, added a little in excess, and boiled it for a quarter of an hour, in order to dissipate the carbonic acid entirely.

Nitrate of mercury precipitated phosphoric acid.

To a portion of the liquor thus prepared he added a few drops of the solution of nitrate of mercury at a minimum; but instead of these giving it a red colour, as is usual with chrome, they threw down a white precipitate, which at first he took for muriate of mercury, but it afterward appeared to be phosphate of mercury.

Limewater threw down more.

Instructed by this trial, he added to the remainder of the liquor limewater, which, when the acid was saturated, produced a flocculent precipitate. This had a slight tint of yellow, which changed to a green on drying, a circumstance that indicated some foreign matter in the phosphate of lime.

Chrome.

Desirous of discovering the cause of this colour, he heated the precipitate red hot in a silver crucible; in consequence of which the green tint, instead of disappearing, became more intense. He then fused a little with borax by the blowpipe, and the fine emerald green colour the salt assumed confirmed his first suspicion of the existence of chrome in the scoriæ from the refining furnace.

Oxide of chrome with a little silex.

The remainder of the precipitate, being treated with nitric acid, was not entirely dissolved; a portion being left of a very deep green colour, which was nothing but oxide of chrome mixed with a little silex, the particles of which being

brought

brought together and hardened by the heat, it had lost the capacity of being soluble.

The solution was void of colour; and oxalate of ammonia threw down from it a granulous precipitate, which when washed and dried weighed 2 decig. [3 grs.], and was true oxalate of lime. Lime.

The liquor from which the oxalate of lime had been precipitated, as has just been mentioned, being evaporated to dryness, and the residuum calcined, yielded an acid, which had all the properties of the phosphoric. Phosphoric acid.

The first liquor, to which the limewater had been added to precipitate the phosphoric acid, was mixed with nitrate of mercury recently prepared; when a brown yellow precipitate was formed, which assumed a green tinge by drying in the air. The precipitate fused with borax gave it a very fine green colour, which proved it to be a chromate of mercury with excess of oxide. Chrome.

Thus the presence both of chrome and phosphoric acid in the scoriæ from the refining furnace is demonstrated. These matters, as well as those that will be mentioned below, existed in the pig iron, and previously in the ore, for nothing was added during the processes of working them, from which these could have been produced. All these must have existed in the pig iron and in the ore.

After the chrome, phosphoric acid, manganese, and a portion of the silex and alumine, had been separated, Mr. Vauquelin dissolved in muriatic acid the ferruginous part, which had then a yellowish red colour. He observed, that, notwithstanding the alkali had taken from it a great deal of oxide of manganese, a perceptible portion of oxigenized muriatic acid was produced, as the dissolution went on. Muriatic acid oxigenized by the ferruginous part.

A white powder remained at the bottom of the liquor, which when washed and dried weighed 88 cent. [13·6 gr.], or about a fifth of the weight of the scoriæ. During the evaporation of the liquor, which was carried to dryness, a portion of the same substance was precipitated, which was freed by means of muriatic acid from a little iron, that fell down with it. This contained some traces of chrome, for it communicated to borax a plain green colour. It was silex. Silex.

Mr. Vauquelin precipitated the iron from its solution by ammonia, Lime.

ammonia, and added to the filtered solution oxalate of ammonia, which formed in it a pretty copious precipitate, that was oxalate of lime.

Manganese, alumine, and lime.

The iron, while still moist and in an attenuated state, was treated with acetous acid, the mixture evaporated to dryness, and the residuum redissolved in water. In the clear and colourless liquor were detected by different means the presence of oxide of manganese, and of alumine, which had escaped the action of the alkali in the first operation, and of a pretty large quantity of lime, which the volatile alkali had precipitated with the help of the oxide of iron.

Component parts of the scoriæ.

From these experiments, and the results they furnished, it is evident, that the dross or scoriæ of the refining furnace, on which they were made, are formed of, 1st, a large quantity of iron oxided at a minimum; 2d, oxide of manganese; 3d, phosphate of iron; 4th, chrome, probably in the state of oxide; 5th, silex; 6th, alumine; 7th, lime, part of which is perhaps combined with phosphoric acid.

All these were in the pig iron.

It can scarcely be doubted, that all these matters were contained, at least in part, in the pig iron that furnished the scoriæ: the charcoal might have imparted to them at most some lime, silex, and manganese; but the analysis of the ores, and of the pig iron itself, will soon instruct us what we ought to think on this point.

Ores examined.

III. *Examination of the bog ores.*

Bog ores of Drambon,

The ores subjected to analysis by Mr. Vauquelin were, 1st, those employed at the forge of Drambon. These are in spherical nodules of different sizes, and some irregular fragments of limestone are observed among them. 2d, those of Chamfont and Grosbois. These much resemble the former. Those of Grosbois contain a pretty large quantity of limestone. 3d, that of Chatillon-sur-Seine. This is of an ochry yellow colour, in grains as small as millet seed, and no limestone is seen among it, but it contains a pretty large quantity of clay.

Chamfont, and Grosbois.

Mr. Vauquelin gives at large his analysis of the ore of Drambon, observing, that the other ores include the same principles, though in different proportions; at the same time

time the quantities he has assigned to its different component parts he gives only as approximations. Quantities only approximations.

Ten grammes [154·5 grs.] of the ore of Drambon, treated with caustic potash, assumed a very intense green colour, that communicated itself to the water in which it was lixiviated. The ore being subjected to the same operation a second time, it produced a similar effect, but less striking. Ore of Drambon.

The liquors were boiled, and 3 decig. [4·6 grs.] of manganese fell down, containing a little silex, and an atom of iron. Manganese, silex, and iron.

The solution retained a slight yellow colour, as in that from the scoriæ; and Mr. Vauquelin, supposing this colour to be produced by the same substance, saturated it with nitric acid. With this liquor he mixed a solution of nitrate of mercury made without heat; when it became colourless, and a white precipitate fell down, that did not give any tinge to glass of borax.

As the liquor contained an excess of acid, it was suspected, that, if any chromate of mercury had been formed, it was held in solution. Accordingly a few drops of a solution of pure potash were added, and a brown red precipitate was obtained, which, being fused with borax, gave it a fine emerald green. This indicated, that it was chromate of mercury, perhaps with a little phosphate of the same metal. Chrome, and perhaps phosphoric acid.

The liquor being still acid, and retaining some mercury in solution, Mr. Vauquelin imagined it still contained chrome. He therefore added a few drops of nitrate of silver, in hopes of obtaining a crimson red precipitate; but what fell down was of an orange yellow, and did not give a green colour to borax. It was phosphate of silver. Potash added to the remaining liquor produced a very bulky, flocculent, lemon-coloured precipitate. This acquired a green hue as it dried, and was chromate of mercury, containing silver, with a small quantity of alumine and silex. Phosphoric acid. Chrome, alumine, & silex.

The mercury was separated from the silver in a gentle heat by means of muriatic acid, diluted with two parts of water, that it might not dissolve the muriate of silver. At once the precipitate became white, and the acid green. The Chrome.

olution

solution being evaporated to dryness left a blackish matter, which gave a very fine green colour to borax.

On treating afterward with sulphuric acid, and precipitating by limewater, Mr. Vauquelin obtained 1·5 per cent of magnesia. Though this earth was found in the pig iron from each of the five bog ores, he does not venture to assert, that it exists in all: but he observes he has much more reason to think, that chrome and phosphoric acid are constantly found in it.

Magnesia.

Reflecting, that oxide of manganese, chrome, and magnesia, which he had just obtained, were found likewise in aerolites, or meteoric stones, he questioned whether it were not possible for iron ores, to have contributed in some way or other to the formation of these stones. This idea led him to examine, whether nickel likewise did not occur in bog ores; but his researches were fruitless.

Similarity of these ores to meteoric stones:

but no nickel in them.

From what has been said it follows, that the bog ores analysed were composed of, 1st, iron; 2d, manganese; 3d, phosphoric acid; 4th, chrome; 5th, magnesia; 6th, silex; 7th, alumine; and 8th, lime. The chrome, phosphoric acid, and magnesia, had not before been noticed in these ores.

Component parts of the bog ores.

IV. *Examination of the iron, that sublimes and collects in the chimneys of the refining furnace.*

This iron is found adhering to the sides of the chimneys of the refining furnace in the shape of stalactites, which are sometimes more than a foot long and three or four inches in diameter. They are formed of agglutinated grains, red in their fracture, leaving great intervals between them, and having but a slight action on the magnet.

Iron sublimed into the chimneys of the furnaces,

We shall not give the particulars of Mr. Vauquelin's analysis, but he concludes it with the following words.

"In this sublimed iron then, there are oxide of manganese, silex, phosphoric acid, and above all a great deal of chrome. These matters therefore have been volatilised by the caloric, either by being dissolved in this fluid, or by yielding to the impulse of the current of air; but in either case they have issued from the pig iron, during the process of refining."

contains oxide of manganese, silex, phosphoric acid, and much chrome.

V.

V. *Examination of the pig iron of Drambon.*

Mr. Vauquelin having found oxide of manganese, chrome, phosphoric acid, and earths, in the scoriæ of the refining furnace, it was natural for him to infer, that he should find the same substances in the pig iron; since it is this, that furnishes these scoriæ, at least for the most part, in the process of refining. This fact was fully confirmed by analysis. Pig iron of Drambon.

He proceeded in the following way. Ten gram. [154·5 grs.] of gray pig iron of Drambon reduced to filings were dissolved in sulphuric acid diluted with six parts of water. The hidrogen gas evolved during the solution was collected. It had an extremely fetid smell, very much resembling that of rotten garlic; but still more that of phosphuretted hidrogen gas, though it had a certain pungency, which the phosphuretted hidrogen has not. The nature of this gas will be noticed presently. Analysed. A very fetid hidrogen gas.

The residuum was of a very deep black, and diffused an extremely strong smell of phosphorus. It weighed 53 cent. [8·2 grs.] or a little more than a twentieth of the iron employed. Residuum.

The upper part of the bottle in which the solution was made, and the tube through which the hidrogen had passed, being so greasy that water would not adhere to them, Mr. Vauquelin suspected, that oil had been formed; a fact first announced by Mr. Proust a few years ago on a similar occasion, and which Mr. Vauquelin adds he had himself observed before that, when dissolving certain kinds of tin. Oil formed.

To know whether any of this oil remained in the residuum of the pig iron dissolved in the sulphuric acid, he boiled it with highly dephlegmated alcohol, and filtered the liquor hot. Residuum treated with hot alcohol,

This alcohol became milky on the addition of water; and being exposed to a gentle heat, drops of oil separated from it as the alcohol evaporated. This oil was clear and transparent; it had a slight yellow tinge; and its taste was hot and a little pungent. It appeared to be of a middle kind between the volatile and fat oils. more oil obtained.

After

The residuum deflagrated with nitre. After the oil it contained was separated from the residuum of the pig iron, this residuum was deflagrated in a silver crucible with a little very pure nitrate of potash, the matter was washed with distilled water, and a light yellow liquor was obtained. This was mixed with a solution of the nitrate of ammonia, to precipitate the silex and alumine presumed to be contained in it; and a small quantity of these was separated. Limewater added to the filtered liquor formed in it a copious precipitate, which had all the characters of phosphate of lime.

Silex, alumine, and phosphoric acid separated.

Chrome. To ascertain whether there were any chrome in this liquor, it was first boiled to volatilise the ammonia, and a few drops of nitrate of mercury were added, which was precipitated of a brown yellow, in consequence of a little lime remaining. This precipitate however gave a green colour to borax, which proves, that it contained chrome.

Lixivium. The lixivium from the residuum of the solution calcined with nitrate of potash then contained phosphoric acid, chrome, and silex mixed with a little alumine. There was likewise in it an atom of manganese.

Residuum treated with muriatic acid. The residuum having been thus treated and lixiviated was in the form of a reddish powder, which was dissolved for the greater part by muriatic acid. There remained however a small quantity of grayish matter, which was silex mingled with chrome, for it gave a very decided green colour to borax.

Iron with silex. The muriatic solution contained a great deal of iron. It assumed the consistence of a jelly on evaporation, which demonstrates, that it contained silex; and it is probable, that a little chrome and manganese too were concealed in it.

Contents of the iron. It appears then, that this pig iron contains, beside carburet of iron, phosphuret of iron, manganese, chrome, silex, and alumine. Next to the iron and carbon, it appeared to Mr. Vauquelin, that the phosphorus was most abundant. It is then in the residuums of the solutions of pig and bar iron that we must henceforward look for phosphorus, rather than in the solutions themselves, as has hitherto been done. Probably the neglect of examining these residuums with sufficient attention is the reason of our remaining

Probable reason of our ignorance of the causes of the badness of iron.

maining so ignorant of the causes of the bad quality of iron.

Mr. Vauquelin however admits, that there is likewise a small quantity of phosphorus converted into acid, and dissolved in the liquor, probably in the state of phosphate of iron, by means of the sulphuric acid. It appears to him, that, when the sulphuric acid is less diluted with water, a larger quantity of phosphorus dissolves in the liquor. To separate this phosphate of iron, he dilutes the solution with seven or eight parts of water, and mixes with it carbonate of potash, till almost the whole of the acid is saturated. A white precipitate is formed, more or less copious according to the kind of iron employed; and at the expiration of a few days it grows yellowish. This precipitate, washed and dried, he treats with potash in a silver crucible at a red heat: he then lixiviates the matter with water, and, after having saturated the liquor with nitric acid, and boiled it to expel the carbonic acid, he adds limewater, which commonly forms a white flocculent precipitate, or semitransparent if phosphoric acid be present.

Phosphorus.

Separation of the phosphate of iron.

He has likewise found a large quantity of chrome in the precipitate produced by carbonate of potash in the solution of pig iron by sulphuric acid. It follows therefore, that chrome as well as phosphorus is oxigenized and dissolved in sulphuric acid.

Chrome oxigenized and dissolved in sulphuric acid.

It is advisable to test the alkaline liquor with nitrate of ammonia, previous to saturating it, in order to know whether it hold any silex or alumine in solution. If it do, a sufficient quantity should be added to precipitate these earths, after which they must be separated by the filter: for without this precaution they would be precipitated by the lime, and might be mistaken for phosphate of lime. Mr. Vauquelin has found very evident traces of this salt in the pig iron of the works at Drambon, though he employed sulphuric acid diluted with six parts of water to dissolve it: there was much less however, than remained in the residuum of the solution. This was the only kind of pig iron he examined, but he conceives it probable, that all the irons from bog ores contain the same foreign matters.

Caution.

VI.

VI. *Examination of the bar iron of Drambon and Pesmes.*

Cold short iron analysed. Mr. Vauquelin dissolved 5 gram. [77·2 grs.] of cold short iron of Drambon in sulphuric acid diluted with five parts of water. Hidrogen gas. He collected the hidrogen gas, that was evolved of during the dissolution, and found it to have exactly the same smell as that of the gas from the pig iron, but not quite so powerful.

Residuum. The residuum left by these 5 gram. was much less copious than that of the pig iron, and appeared likewise not to be of so deep a black. While it was wet it emitted a very strong fetid smell, analogous to that of hidrogen gas. It weighed 15 cent. [2·3 grs.], amounting to 3 per cent. The solution of the iron had the same smell, which was not dissipated but by evaporation.

Phosphorus. A few particles of this residuum, thrown on a burning coal, emitted a white vapour, with a smell resembling that of arsenic and phosphorus. Heated red hot in a silver crucible, it burned with flame, and left behind a yellowish powder. This was mixed with a little caustic potash, calcined, and lixiviated. The liquor being filtered, saturated with nitric acid, and subjected for a few minutes to heat, limewater was added, which threw down a white flocculent precipitate, consisting chiefly of phosphate of lime, but with an atom of silex and perhaps of alumine.

It is certain from these experiments, which Mr. Vauquelin repeated several times, that the iron of Drambon, though it is considered as of pretty good quality, contains very perceptible traces of phosphorus. He likewise found some slight traces of it in the solution by sulphuric acid.

Iron of Pesmes of better quality. The iron of Pesmes afforded nearly the same results. The residuum however was less by one half, amounting only to 1½ per cent; and it contained less phosphorus. This iron is very tough, and is reckoned one of the best in Franche-Comté.

VII. *Of the hidrogen gas.*

The fetid hidrogen gas. Various experiments, which Mr. Vauquelin made by the help of oxigenized muriatic acid on the hidrogen gas evolved from

from the pig and bar iron, led him to conclude, that phosphorus is the chief cause of its fetid smell.

VIII. *Recapitulation and inferences.*

From the experiments I have related, says Mr. Vauquelin, it follows: General conclusions.

1. That the five sorts of bog ore I analysed are composed of the same principles, which are, beside iron, silex, alumine, lime, oxide of manganese, phosphoric acid, magnesia, and chromic acid.

2. That the five sorts of ore having been taken at a venture from places tolerably distant from each other, it is probable, that all ores of the same kind contain the same substances.

3. That these ores want only nickel, to contain the same substances, as the stones that have fallen from the atmosphere.

4. That part of these substances remains in the pig iron, and probably in larger quantity in cast iron, which may be the cause of its greater hardness and brittleness.

5. That the greater part of these substances is separated during the refining of the pig iron, when this operation is well executed; since they are found in the scoriæ, and in the sublimed iron that adheres to the insides of the chimneys of the refining furnaces.

6. That traces of them however are found in bar iron of good quality; and that probably chrome, phosphorus, and manganese, are the chief causes, that render iron hot short or cold short.

7. That the process of refining merits the greatest attention from iron-masters; since it appears, that the good quality of iron depends on its skilful execution. The quality of iron depends on its refining.

8. That the presence of phosphorus and of chrome is to be sought for not in the solutions of pig and bar iron alone, but also in the residuums of their solutions.

9. That by the union of hidrogen and carbon during the dissolution of iron, and particularly of gray cast iron, an oil is formed, which, in conjunction with a small quantity of phosphorus, communicates a fetid smell to the hidrogen gas that dissolves them.

10. That

10. That it is to these two substances the hidrogen gas owes its properties of burning with a blue flame, and being heavier than when pure.

11. Lastly, That the oil and the phosphorus are separated from the hidrogen gas by oxigenized muriatic acid, which destroys them.

VI.

On the Maddering of Cotton and Linen Thread, and Dyeing them Adrianople Red and other fixed Colours; and on Spontaneous Inflammations: by John Michael Haussmann*.

Fixing colours on thread.

In order to proceed to the dyeing of cotton and linen thread all sorts of fixed colours, nothing is necessary, but to fix on the thread, in any manner whatever, more or less alumine, after having given it a slight coating of oil. The complete success of the result however depends on certain modifications to be observed in the processes.

Oils do not remain mixed with alkaline solution of alumine so well in large quantities.

The various experiments I had made in the art of dyeing had rendered me so familiar with trials on a small scale, that at length I found none of them fail. It is not till since my paper on maddering was published in the Annales de Chimie, that I experienced difficulties in the application of oils, when operating in a larger way. The linseed oil, which had always afforded me a milky mixture in limited proportions with the alkaline solution of alumine, then speedily separated, when I was desirous of making a pretty large stock, and the impregnation of the skeins became impracticable under these circumstances. It was the same with all the other fat oils: fish oil, indeed, continues mixed a pretty long time, but its smell is too offensive.

Fish oil best.

Drying oils tryed with success.

To remedy the inconvenience of the separation of the oil in the alkaline solution of alumine, I had recourse to drying oils, or those boiled with metallic oxides. Linseed oil, boiled with ceruse, minium, or litharge, by means of water

* Annales de Chimie, vol. XLVIII, p. 233.

to

to prevent its combustion, dissolves a good portion of oxide of lead, and continues mixed with the alkaline solution of alumine in a milky form, as long as is necessary for the impregnation of the skeins. If this mixture be used in the proportions and manner pointed out in my memoir, and following strictly in every other respect the process as I have described it, fine and permanent colours cannot fail to be obtained. However, notwithstanding the simplicity of the process, I can no longer recommend its use, because it has exposed me to the danger of a fire, and I will relate in what way.

But dangerous.

In order to see whether red cotton, which was not sufficiently fixed, might be rendered so by impregnating it with a mixture of an alkaline solution of alumine and boiled linseed oil, containing an excess of the oil, drying it, and then boiling it a very long while in bran water, I mixed the alkaline solution of alumine in the proportion of an eighth, a twelfth, and a sixteenth of boiled linseed oil. With this mixture I impregnated a few hanks of dyed cotton, which, after being left to dry a whole summer's day in the open air, were laid on a rush bottomed chair, that stood in the window of my closet. Finding myself indisposed that day, I went to bed at seven o'clock. My children went into my closet for some papers, an hour after I had left it, and perceived no heat or smell in the cotton, to indicate a commencement of burning. All the workmen had gone to bed, and were fast asleep, when one of the watchmen of the bleaching ground, seeing a great light in my closet, gave the alarm of fire, and roused us all between twelve and one o'clock. My sons, knowing that I was not able to get out of bed, and unwilling to lose time in searching for the key, broke open the door of the closet, which was in a detached, uninhabited building. They went in, notwithstanding the thick smoke and insupportable smell of the oily combustion; and found the chair with the cotton burning so furiously, that the flames rose to the ceiling, and had already cracked the glass, and set fire to the window-frame. They at once presumed, that this commencement of a fire could proceed only from the spontaneous inflammation of the cotton impregnated

Cotton thus impregnated took fire spontaneously.

pregnated with boiled oil, since no one ever went into the closet with a lighted pipe, or any thing else burning.

This not believed by many. Tried again.

As I found, that several persons belonging to the manufactory did not credit this explanation, I again impregnated a few dozen hanks of some old cotton, that had not been well dyed, in the same manner as I had done the cotton that was burned. These I set to dry in a simi ar manner in the open air; and as it threatened to rain, ordered them to be hung upon a line under a penthouse, directing one of the watchmen to look at it every quarter of an hour during the night, and throw it into a bucket of water, as soon as he perceived it begin to heat. But this man could not believe the possibility of the cotton's taking fire of itself, as he afterward confessed to me, and walked through the manufactory without once looking at the penthouse. At length however he returned to lie down, and found by the great light he saw, that what I had foretold in case he was negligent had taken place. Finding the cotton as well as the line was burned, he took the bucket of water to extinguish the posts, which were already on fire.

Took fire as before.

Experiments on spontaneous combustion.

Though these two accidents did not at all surprise me, I could the less forgive myself for the first, as, in order to prevent similar accidents, I had made some experiments on spontaneous combustions at a public house fifteen years before. On that occasion I had spoken of the probability of fires being occasioned by heated substances, or substances that have a tendency to heat, and which are thoughtlessly put in places capable of being set on fire. The substances I mentioned to those of the company, who were not sufficiently acquainted with the phenomena of spontaneous combustion, were roasted coffee and chocolate nuts; fermented plants; ointments made with metallic oxides put hot into wooden barrels; bales of raw cotton, as well as woollen yarn or cloth packed up warm, and even linen when ironed, and put away in drawers yet hot; and lastly substances of every kind impregnated with boiling oil, as silk or cotton. I showed them besides, that in all circumstances where the oxigen of the atmosphere is rapidly attracted and absorbed by any cause, the caloric or heat, which serves as a base to the oxigen, and gives it the properties of a gas, is given out in

Substances liable to it.

Owing to rapid attraction of oxigen.

in such abundance, that, if the absorbing substance be capable of taking fire, or surrounded by inflammable matters, spontaneous combustion will take place.

To confirm what I had said of the theory of these sorts of combustions to those present, who were not familiar with chemical operations, I performed the following experiments. 1. The inflammation of a mixture of sulphur and iron filings kneaded with water. 2. That of boiled linseed oil by highly concentrated nitric acid. 3. That of phosphorus by atmospheric air, as well as in pure oxigen gas, placed for this purpose on a china saucer over boiling water, in order to separate its particles by fusion without having recourse to rubbing it. 4. That of phosphuretted hidrogen gas by the contact of the atmosphere, an imitation of the Jack with a lantern. 5. The combustion of pyrophorus, thrown into the open air, and into pure oxigen gas. 6. The reduction of roasted bran, put hot into a coarse bag, to an ignited coally mass by the action of the atmospheric air.

Experiments in confirmation of this.

I was not ignorant, that essential or volatile oils become resinous, and that drying oils boiled with metallic oxides grow thick and even hard by their combination with oxigen; and this was the reason why my hanks of cotton impregnated with a mixture of boiled linseed oil were exposed a whole day to the air, hung separately on poles: but I supposed they were then saturated with oxigen, and consequently incapable of occasioning the least accident. I felt myself so secure in this respect, that I have several times dried a great deal of oiled cotton in hot rooms; and it was owing to chance alone, that it was never put together, till the moment when it was washed in order to be dyed. It is possible however, that the proportion of a thirty-third part of boiled linseed oil mixed with the alkaline solution of alumine might be insufficient, to excite spontaneous combustion in the hanks put together after being dried. If therefore a person were inclined to employ a mixture of boiled linseed oil and the alkaline solution of alumine, on account of the simplicity of the process, he should take the precaution, to let the hanks remain spread separately on the poles, till the instant of their being washed previous to dyeing; which, in conjunction with the brightening, would remove all the excess

Attraction of oxigen by oils.

That in the cotton supposed to be saturated with it.

Owing in part perhaps to the proportion of oil employed.

Precaution.

cess of oil, leaving none but what was completely saturated with oxigen, and then there would be nothing to fear.

Simplest brightening for Adrianople red.

Since the publication of my memoir, I have likewise satisfied myself, that the simplest brightening for Adrianople red, by which the brightest and most lasting colours are procured, consists merely in boiling for a very long while with bran-water in a covered boiler, with a tube in the middle of the cover, to let out the steam, and prevent the bursting of the vessel. Care muft be taken however, to change the water as often as it grows red, which will be two or three times in the beginning of the boiling; otherwise the thread would be continually taking up the dun particles, which the bran-water had removed, and a bright colour could not be obtained.

Process without danger.

All danger indeed might be avoided, without much deviation from the simplicity of my process, whether the hanks were heaped up or not. Nothing more is necessary for this, but to give it a coat of olive oil in a very attenuate state, at two different times, after having well steeped it in an alkaline lixivium, washed, and dried it. For this purpose a lixivium of the subcarbonate of potash or of soda is to be made of the strength of 1° or $1\frac{1}{2}^{\circ}$ on the saltpetre areometer. This must be tried, by mixing with it a few drops of olive oil, to see whether these produce a milky mixture, or rise and float uncombined on the top; for as the alkali may contain more or less foreign matter, the lixivium must be weakened, or strengthened by an addition of alkali, as it is absolutely necessary, that it should assume a milky appearance on the trial with oil. When the lixivium is of a proper strength, thirty-two parts are to be mixed with one of olive oil, at first by little and little, and afterward more quickly, stirring it continually the whole time. This milky mixture keeps pretty long, and if the oil begin to rise to the top in the form of cream, the mixture must be stirred afresh. The impregnation of the thread ought to be entrusted to workmen who are most expert in this process, because the accurate distribution of the oily parts has great influence on the evenness of the colour. Each workman should take only a sufficient quantity of the milky mixture in any kind of vessel, so as to be able easily to work it with all possible dexterity

as

as many hanks as he can wring out with facility. Thus he will go on, taking constantly the same number of hanks, and the same quantity of milky mixture. What he wrings out he will put into a separate vessel, and restore to it by his eye the quantity of oil the thread has absorbed, if the trifling value of this residuum, which will contain but little oil, do not induce him to throw it away. The impregnation may be performed in the whole quantity of milky mixture, but then the quantity of olive oil, that the hanks have absorbed, will continually require to be replaced by the eye, as soon as the intensity of the milkiness appears to be diminished: the art of doing this however is easily acquired by practice. After having dried all the hanks together, they are to be impregnated a second time, in the same manner as before, but without washing them first: and when they have been again dried, they may be impregnated without previous washing, once, twice, or three times, with the pure alkaline solution of alumine without oil, in the manner described in my memoir. When they come to be dyed the colour will be more or less deep, in proportion to the number of impregnations.

To obtain light tints however, and at the same time even, it is better to impregnate them three times, weakening the alkaline solution of alumine proportionally. The thread might also be impregnated with this solution, either strong or weak, three times following, without previous washing; which would greatly diminish the number of operations, that are certainly tedious and troublesome: but in this case the solution must be examined from time to time, to see whether what the impregnated and dried thread discharges into it do not render it too strong. For light tints.

On redyeing red colours, it must be recollected, whether they were brightened by boiling in bran-water, or by means of soap and alkalis. In the first case they grow deeper by attracting the colouring particles of the madder; in the second they are weakened, and lose their excess of alumine, without which repeated dyeing produces no effect. The removal of this excess of alumine may be prevented, by substituting for soap and alkalis, in order to produce crimson tints, a portion of the alkaline solution of alumine, which is to be added to the bran-water toward the end of the brightening. The true Redyeing reds. Real Adrian-

ople reds re-dyed are browned by lie.

true Adrianople reds become much deeper on dyeing again, and are then browned by the test of boiling in a lixivium of wood-ashes. Before they are redyed, this changes them very little. In general reds are browned to more or less disadvantage, in proportion to the longer or shorter time they have been boiled in the brightening. As the real Adrianople reds have a strong smell, the Turks perhaps use fish oil, which they add directly to the alkaline solution of alumine, or mix with a very weak lixivium of alkaline carbonate.

Turks use fish oil.

Process admits of great variations.

The processes for Adrianople reds may be infinitely varied; for in whatever manner, and by whatever acid or alkaline solvents the alumine is fixed in the thread, after having given it a slight coating of any kind of oil, we cannot fail to obtain reds more or less bright, in proportion to the care employed in maddering and brightening.

Oils mix with a weak solution of alkaline carbonate, not with a strong.

The reason why the oils, which very easily combine with caustic alkalis and form soap, do not mix with concentrated lixivia of alkaline carbonates, while with the same lixivia greatly diluted they form a kind of artificial milk, appears to be the more difficult to explain, as we might at least suppose, that there is a tendency to combination in these milky mixtures. A simple suspension of the integrant particles of the oil, that should take place in the diluted lixivium preferably to the stronger, is not more explicable.

Mistake corrected.

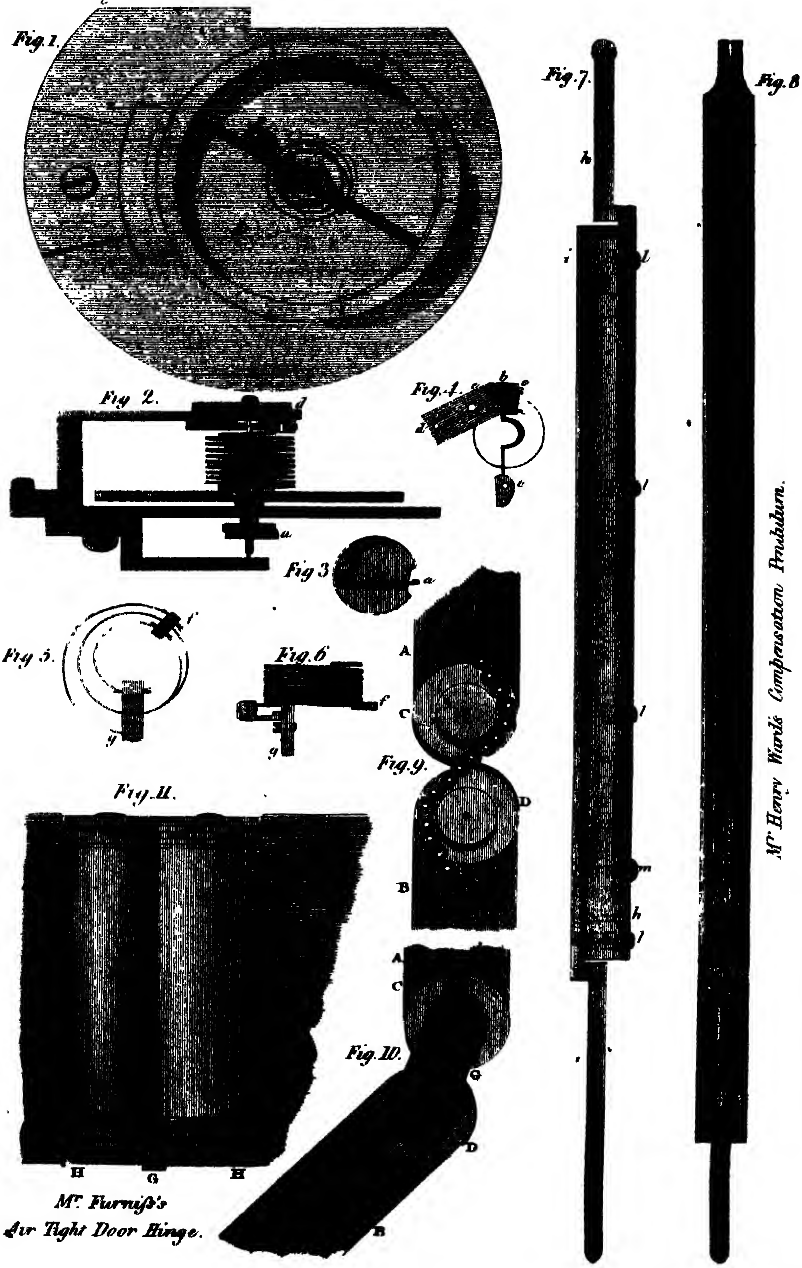
It remains for me to apologize for a misstatement I had made with regard to the fabrication of the true Adrianople red cotton used in the manufactories. What was shown to me was of very inferior quality; but I have since seen some of the finest and most permanent dye: hence I conclude, that the manufacture of the Turks, like that of all other nations, is according to the price the purchaser will give for it.

Query whether soda tend to produce the spontaneous combustion, or alumine to prevent it.

I must not omit to observe likewise, that among the cotton I had burned, there was some both times, that had been impregnated with the mixture of weak lixivium of carbonate of soda and boiled linseed oil in the proportions of an eighth, a twelfth, and a sixteenth part. It remains to be proved, whether this cotton will take fire sooner than that, which is impregnated with a mixture of the alkaline solution of alumine and boiled linseed oil in the same proportions.

As

Mr. Hardy's correction of Vibration in Time Keepers



Mr. Furniss's Air Tight Door Hinge.

Mr. Henry Ward's Compensation Pendulum.

As this last mixture is capable of attracting in some degree the humidity of the air, I should rather think, that the cotton treated with the first is more liable to take fire*.

The experiments I have continued to make on the use of galls, in the manufacture of Adrianople red, lead me to believe, that the alumine is fixed in the cotton in consequence of the formation of a gallate of alumine, from which the gallic acid is afterward attracted by an alkaline carbonate previous to the dyeing. As soon as I have satisfied myself of the truth of this supposition, I will not fail to publish an account of my experiments.

In galling perhaps gallate of alumine formed, and after decomposed by the alkalis.

VII.

Account of Inventions for equalizing the long and short Arcs of Vibration in Timekeepers; by Mr. WILLIAM HARDY, *No. 29, Coldbath Square*†.

THE equalization of the time of different arcs of vibration of the balance of a time keeper having lately given rise to much discussion, I beg leave to offer for the approbation of the Society three different modes of obtaining this end.

Equalization of arcs of vibration effected three ways.

The first method is by a straight spring placed edgewise across the diameter of the impellent pallet *a*, Pl. II, fig. 2 and 3, and screwed at the end opposite to the direction of the wheel, on its approach toward the centre of this pallet; at the other extremity of this spring is a flat face, or curved surface, to receive the approaching tooth of the escape wheel, which gives the impulse; this spring acts between two pins placed in the pallet near its end. By reducing this spring to a certain degree of strength, so that it may yield a little to the force of the wheel in giving the impulse, the different vibrations will be performed in the same time; but the proper degree of strength can only be determined by repeated trials. This method possesses besides this farther advantage, that the acting surfaces are not so liable to be injured

1st method.

Advantage.

* Perhaps not, for this very reason. T.

† Transact. of the Society of Arts, vol. XXV, p. 113. The silver medal of the Society was voted to Mr. Hardy for this invention.

jured by the drop of the wheel upon the spring, as upon a solid surface, nor the vibrations of the balance so much disturbed by the impulse.

2d method. The second method is by a straight spring *b c*, fig. 1 and 4, screwed to the under part of the cock, placed edgewise and diametrically over the cylindrical spring, and having a piece cut out to clear the arbor of the balance. This straight spring is at one extremity fastened to the end of the pendulum-spring, and, at the other extremity, its elasticity is reduced so as to yield a little before the pendulum-spring operates. On the opposite of the cock, where the spring is screwed, is fixed a stud *d* projecting downward,and having a slit to admit the small piece at the end of the spring *b*. On each side of this slit is an adjusting screw *e e*, the points of which face each other, and are placed so as that the spring may move equally between them from its point of rest. The action of the spring between the adjusting screws requires to be somewhat less than the angle of escapement. Let the balance be made to vibrate, so that the straight spring may move up to the adjusting screws upon each side, and no farther; being weaker than the pendulum-spring, its exertion will be less; hence the time of the vibrations will be prolonged, but as they increase, the exertion of the pendulum-spring will commence and progressively accelerate them, and this acceleration will always be in proportion as the exertion of the pendulum-spring is to the action of the straight spring between the two adjusting screws. Thus it will always counteract the accelerating effect of the escape-wheel in the small arcs of vibration, so that the whole of them shall be performed in the same time.

3d. method. The third method is by connecting a piece of short spring-wire to the pendulum-spring by a small piece *f*, fig. 5 and 6, with two holes; pinning the two springs together about half a turn from the stud of the pendulum-spring; and clamping the other end of the short spring at its natural point of rest to a sliding piece, *g*, which projects out from the pendulum-spring stud. By this manner of fastening, both springs will act together, and each will retain its natural point of rest; but by moving the sliding piece, which clamps the end of the short spring, and placing the spring

a little

a little on the strain, in opposition to each other's exertion, the point of rest of both springs will be destroyed. Thus by producing this counteracting force in the two springs at the lowest point of vibration, the accelerating effect of the escape wheel upon the balance in the small arcs of vibration will be corrected, whereby the whole of them will vibrate in equal time.

Extract of a Letter from Captain William Brown, addressed to Mr. John Nichols, Millpond-bridge, Bermondsey.

Respecting the chronometer which I purchased from Mr. William Hardy last year, the jolting of the coach in the conveyance to Liverpool altered its rate of going to 34″ slow, which rate it continued so exactly, that in making Cape de Verd, on the coast of Africa, (the longitude of which has been correctly ascertained) in 24 days from Liverpool, and carefully measuring my distance from the Cape, I could not discover it to have deviated from the rate, say 34″ slow, one second in the whole time; and I have every reason to believe that it continued the same rate, until my misfortune, when it got immerged in sea-water, having lost my ship on a shoal five or six leagues from the Riopongas, this dangerous bank not being laid down correctly, or the latitude or longitude given in order to avoid it.

Testimony of the performance of Mr. Hardy's chronometer.

Dangerous shoal.

VIII.

Description of a Compensation Pendulum for a Clock, or Timepiece, with Experiments. By Mr. HENRY WARD, *of Blandford, in Dorsetshire*.*

SIR,

HEREWITH I send you a new compensation-pendulum, which I beg you will lay before the Society of Arts for their inspection. I trust their liberality will be equal to the advantages that may be seen to result from it, together with their consideration of the pains I have bestowed in making it

New compensation pendulum.

* Trans. of the Society of Arts, vol. XXV, p. 116. The silver medal of the Society was voted to Mr. Ward.

it. It has cost me much labour, time, and expense; indeed, it has occupied almost the whole of my attention for the last nine months.

If any objections should be made to it, I will endeavour to answer them, and make any further experiments required.

I am Sir,

Your obedient servant,

HENRY WARD.

Description of it.

Pl. II, fig. 7, *h h i i*, are two flat rods of iron or steel, about half an inch wide, and an eighth of an inch thick. *k k* is a rod of zinc interposed between them, and is nearly a quarter of an inch thick. The corners of the iron rods are bevilled off, that they may meet with less resistance from the air; and it likewise gives them a much lighter appearance. These three rods are kept together by means of three or four screws *l l l l*, which pass through oblong holes in the bars *h h k k*, and screw into the rod *i i*. The rod *h h* is connected to the rod *k k* by the screw *m*, which I call the adjusting screw. This screw turns in the rod *h h*, passes through the zinc rod *k k*, and screws into the iron rod *i i*. The rod *i i* has a shoulder at its upper end turned at right angles, and bears on the top of the zinc rod *k k*, and is supported by it. It is necessary to have several holes for the screw *m*, in order to adjust the compensation. See fig. 8.

Its action.

Now it is evident, that if any degree of heat or cold be applied to this compound rod, the one of zinc expands and contracts as much as the two iron ones together; the distance from the point of suspension to the centre of oscillation therefore must remain the same.

Smeaton's expansion table used.

In proportioning the length of the rods, I made use of Mr. Smeaton's table of expansion of metals, in the 48th vol. of the Philosophical Transactions: where he shows, by experiments made with a pyrometer, that the expansion of iron is to that of unhammered zinc, with the same degree of heat, as 151 to 353, and to that of zinc hammered half an inch per foot, as 151 to 373. This great expanding property

perty of zinc renders it in theory extremely fit for the purpose of compensation in a pendulum, and I was desirous of knowing if it would answer in practice, and likewise the exact proportion, that was requisite to answer the intended purpose.

I made two regulators, the pendulums of which were composed of iron and zinc, as above described; with this difference, however, that one had a detached scapement of a particular construction; the zinc rod was not hammered, the ball of a lenticular form, and weighed twenty pounds; its arc of vibration nearly five degrees. The other had a simple remontoiring scapement, the zinc rod was hammered half an inch per foot, the ball, of a spherical form, weighed forty-six pounds, and vibrated two degrees and three quarters.

Two regulators made for trial.

These regulators were both placed in the same room, and their cases firmly fixed to the wall; the pendulums were suspended from a stout brass cock, screwed to the back of their respective cases. In the inside of each case, and immediately behind the pendulum rod, was hung a thermometer, for the purpose of comparing the degrees of heat. I adjusted them to mean time nearly by corresponding altitudes of the sun. After having compared them together for several days, I found, that the one which had the hammered zinc rod went somewhat faster when the air of the room was heated by a fire in the grate than the other did. Hence I concluded, that the difference of expansion of hammered and unhammered zinc was greater than Mr. Smeaton made it, at least it appeared so in this instance.

Difference of expansion rather greater than Mr. Smeaton's table.

But to determine whether the length of the hammered zinc rod was accurately proportioned to that of the iron ones, without being obliged to wait that length of time that nature would require to produce a sufficient alteration in the temperature of the air, I proceeded to make the following experiment: I caused to be made a tin tube six feet long, and two inches and a half diameter at its larger end, from which it gradually tapered to the other, which was only half an inch diameter. Within the case, and as far from the pendulum as possible, I placed this tube, the smaller

Contrivance for heating the air round the hammered zinc rod.

smaller end was carried through a hole in the top of the case, and projected a few inches above it. In the lower end of the tube was inserted the nozzle of a lamp, and immediately under it, in the bottom of the case, was a hole of an inch diameter to supply the lamp with air. By this means the tube would communicate as much heat to the internal air, as to raise the thermometer about thirty-five degrees.

Previous to the lamp being put into the case, I made both pendulums vibrate exactly together, and after an interval of twenty-four hours, the one with the hammered zinc rod had gained, as near as I could judge, one tenth of a second. The mean height of the thermometer was fifty-three degrees. I now lighted the lamp, and in about four hours every part appeared to be thoroughly heated, and the thermometer arrived at its maximum, which was eighty-eight degrees; at this point it continued with little variation. While the heat was increasing I found the motion of the pendulum was accelerated. I again made them beat exactly together, and in about ten hours after, the heated pendulum had gained one second; the thermometer in the other case continuing nearly the same. The lamp was then taken out, and as soon as the parts were cooled, and both thermometers showed the same degree, I adjusted the beat of the pendulums as before, and, at the end of twenty-four hours, I found the pendulum that had been heated kept precisely the same rate as it did before the experiment was made.

The motion accelerated.

By this experiment the zinc rod was evidently too long, and that by a considerable quantity. The pendulum was then taken down, to have more holes made for the adjusting screw, and after many repeated trials with the lamp and tube, as before, I found the length of the zinc rod to be 22 inches, and consequently the length of the iron ones together $39{\cdot}2 + 22 = 61{\cdot}2$ inches, or, the expansion and contraction of iron to that of zinc hammered half an inch per foot, as 151 to 420.

The pendulum adjusted afresh.

Ratio of expansion between iron and hammered zinc.

Having thus far satisfied myself with the hammered zinc rod, I proceeded to make similar trials with the one that was unhammered; in doing which a circumstance occurred, that

When the air was rarified the arc of vibration was less.

that I cannot account for; when the air in the case was rarified by means of the lamp and tube, the arc of vibration would be about half a degree less than it was before the lamp was applied, which is directly contrary to what I should expect would have taken place. I afterwards found, that the other pendulum was affected the same way, but in an extreme small degree, which, without doubt, was in consequence of the ball being much heavier, and vibrating a smaller arc. In taking the rate of the clock when the lamp was in the case, I at first computed from theory the errour that would arise by such a diminution of the arc, and allowed for it accordingly; but doubting whether the unlocking of the swing wheel might not form a decrease of velocity in the pendulum, and have a greater tendency to retard its motion, I therefore thought the experiment would be rendered more accurate, if the maintaining power was increased until the arc of vibration should be the same. After several trials, I found the length of the unhammered zinc rod to be about twenty-nine inches, which agrees pretty nearly with Mr. Smeaton's experiments; that is, in regard to the relative expansion of iron and unhammered zinc.

Mr Smeaton's ratio between iron and unhammered zinc near the truth.

The zinc rod of the pendulum, which I here send to the Society of Arts, was hammered three quarters of an inch per foot; and by making experiments with it as I had done with the other two, I found the length of it to be twenty-two inches, which is exactly the same length as the one that was hammered half an inch per foot, so that it seems nothing is gained after hammering it to a certain degree; but I cannot think, that any rule can be laid down to enable us to judge of the degree of expansion that will take place with a determinate increase of heat, from the quantity that is extended by the hammer; much depends on the degree of curvature and polish of the stake and hammer, and probably on the heating of the rod at the time; for it is necessary to heat it a little hotter than boiling water, otherwise it will crack in hammering.

Farther hammering the zinc makes no alteration.

Quantity of extension by the hammer no rule.

In all these experiments it is to be understood, that the ball of the pendulum was suspended by its centre; but if the ball be made to rest on its lower edge, the expansion

Ball suspended by its centre.

expansion and contraction of it must be taken into consideration.

Supposed objection to zinc unfounded.

It has been the opinion of some mechanists, that zinc is an unfit substance for a compensation-pendulum, because they have thought it too soft for the purpose, and that after being heated or cooled to a considerable degree, it does not return to its original dimensions. If that was really the case, no doubt but it would be a general one common to all metals in a greater or less degree; but from the experiments and observations I have made on zinc pendulums, I am fully satisfied there is no foundation whatever for such an opinion.

Pendulum at first continually retarded.

Some time in the latter part of last summer, I however noticed a circumstance, that made me doubt the matter—for when I first used my zinc pendulum, I never could bring the clock to keep the same rate two days together, but it was continually retarded, whether I used the lamp or not; and had I not before observed a similar effect on a lever pendulum, that was made of brass and steel, I should have ascribed the cause wholly to the softness of the zinc rod; but by constantly comparing its daily rate with one that had been going a longer time, I found this retarding property gradually wore off, and in less than a month would become quite settled to the rate that it would afterwards keep. By subsequent experiments with the lamp too, I have constantly found, that all the pendulums I have hitherto tried kept precisely the same rate, both during the time they were heated (provided they were properly adjusted) and afterwards, as they had done before.

This common to others.

Owing to the effect of the ball on the point of contact.

The cause of this retardation appears to me to be, that the points of contact of the different pieces, which compose the pendulum, are more closely connected after a little time, than they are at first; that is, those points of contact do, by the weight of the ball, yield to each other in a small degree, until they get a broader bearing.

Advantages of this pendulum.

The advantages of this pendulum are, 1st, that from its simplicity it will never fail to have the desired effect. 2dly, That no extraordinary care is requisite in executing it. 3dly, That the compensation may be increased or diminished with the greatest ease, without stopping the clock more than a minute, by making fast one of the screws that keep the

rods

rods together while the adjusting screw is removing, taking care to release it again afterwards; and 4thly, That it can be manufactured for less expense than any other compensation pendulum hitherto published.

N. B. The compensation of this pendulum which I now send to the Society of Arts is properly adjusted, at least very near the truth. The holes for the adjusting screw are made at such a distance from each other, that by removing the screw one hole, it will produce an alteration in the going of the clock of about a quarter of a second per day with a change of thirty degrees of Fahrenheit's thermometer.

SIR,

PERMIT me to state to you the observations I have made since my compensation-pendulum was laid before the Society.

The regulator, with the hammered zinc rod, and ball of forty-six pounds weight, was firmly fixed to a brick wall at the top of my house. The adjustment of the length of the rods, by means of a lamp, was repeated as before. There was, however, an alteration necessary to be noticed; the ball of the pendulum rested on its lower extremity, instead of being suspended by its centre. I prefer this method, as being less liable to errour, if the rods should be affected by heat or cold quicker than the ball. The length of the zinc rod, as ascertained by the lamp, was now found to be $20\frac{1}{4}$ inches.

Pendulum with the ball resting on the lower extremity.

Length of zinc.

The clock was then set to mean time, and suffered to go without alteration; the result is exhibited in the following table.

1806	Errour of Clock at time of observation.	Number of Days between the Observations.	Daily rate.	Rate of going.
March 21	0″ 0	18	+ 0″· 15	
April 8	+ 2· 8	32	+ 0. 18	
May 10	+ 8· 7	16	+ 0· 80	
26	+ 21· 5	26	+ 1· 10	
June 21	+ 50· 0			

Increased

Length of zinc increased.

Increased the compensation for heat and cold, 6 holes =4¾ inches, or, the length of the zinc rod to 25 inches. The clock was again set to mean time.

Rate of going.

July 1	0″	0	26	— 0″ 36
27	— 9·	3	13	— 0· 21
Aug. 9	— 12·	0	7	— 0· 31
16	— 14·	2	28	— 0· 34
Sep. 13	— 24·	0	12	— 0· 80
25	— 35·	5	22	— 0· 84
Oct. 17	— 52·	1		

Although a thermometer was attached to the clock, I could not from a necessary attendance to business register it regularly; the difference of its height in March and June may be taken at about 22 degrees, and in July and October 14, without much errour.

Proper length of zinc.

On comparing it with the rate of the clock, the compensation, in the latter case, appears nearly as much too great, as it was in the first too small. The true length of the zinc rod ought to be about 23 inches.

The length of the zinc rod, thus ascertained, is 1¾ inch more than the experiment by the lamp makes it; indeed, I have always suspected there might be some errour in that experiment, on account of the length of the arc of vibration being affected by it.

Having no means of finding the time accurately but by equal altitudes, I could not get so many observations as might be wished. I trust, however, these will not be found altogether useless.

I am Sir,

Your obedient servant,

HENRY WARD.

IX.

IX.

Account of a new airtight Hinge for a folding Screen, or for a Door; by Mr. MARTIN FURNISS, *No.* 128, *Strand**.

SIR,

THE model I have herewith sent is my invention. I beg leave to lay it before the Society for the encouragement of Arts, Manufactures, and Commerce, in the hope, that they will be pleased to examine it, and find it worthy of some mark of their approbation. It is a model for putting together the joints of a folding screen, so as to fold in either direction without admitting the smallest quantity of air; it may likewise be appropriated to hanging of doors.

Joint for a folding screen, or door, to exclude air.

I am, Sir

Your humble servant,

M. FURNISS.

A Certificate from Messrs. Wilsons, cabinet-makers in the Strand, testified, that Mr. Furniss's model for screens or doors is his own entire invention, and has been executed by them on a high folding screen for a lady in Baker street, Portman square.

Has been tried.

Reference to the Engraving of Mr. Furniss's Airtight Hinge for a Door or Screen.

Pl. II, fig. 9. A plan of the joint: A B, two sides of the screen with circular ends, joined by a piece of leather reaching from top to bottom fastened at C, and wrapping (like the letter S) partly round the curve of one fold of the screen, and partly round the other to D, where it is also

Description of the joint.

* Trans. of the Society of Arts, vol. XXV, p. 126. Ten guineas were voted to Mr. Furniss for this invention.

fastened;

fastened: E F, a chain formed of brass plates rivetted together, winding round in a groove from off one fold of the screen on to the other, the contrary way to the leather, so as mutually to keep each other stretched tight, the chain winding on when the leather winds off, and vice versa; thus they move smoothly round one another. G, fig. 10, a piece of brass (left out in the last figure in order to show the chain) screwed to the centre of each curve of the screens which forms the hinge, and by keeping the folds of the screen at their proper distance secures the easy action of the chains and leather, and prevents their being overstretched. H H, a line of green twist fastened along the bottom of the screen, and passing through a staple on the joint at G, serving to keep the screen airtight on the floor.

Fig. 11 is an elevation showing the top and bottom joints, with the same letters of reference.

REMARK.

It would frequently be a desirable convenience to have the doors in the interior parts of a house so contrived, as to open either inwardly or outwardly. Mr. Furniss's hinge would effectually answer this purpose: but it would be proper to have the opposite edge of the door padded, as it could not be made to fit tight, and there must be no ledge for it to abut against. A piece of leather nailed on it, and then stuffed with wool or horse-hair, might be so adapted, as to make this side airtight also, at the same time that it would open and shut freely. It is probable however, that some inconvenience might be felt in applying Mr. Furniss's hinge to a door, particularly if large and thick, from the strain upon it by the weight occasioning it to drag. The best remedy for this would be a couple of castors in the foot of the door, one near each end. C.

X.

Dr. Traill's Mercurial Exhausting Machine

Fig. 1.

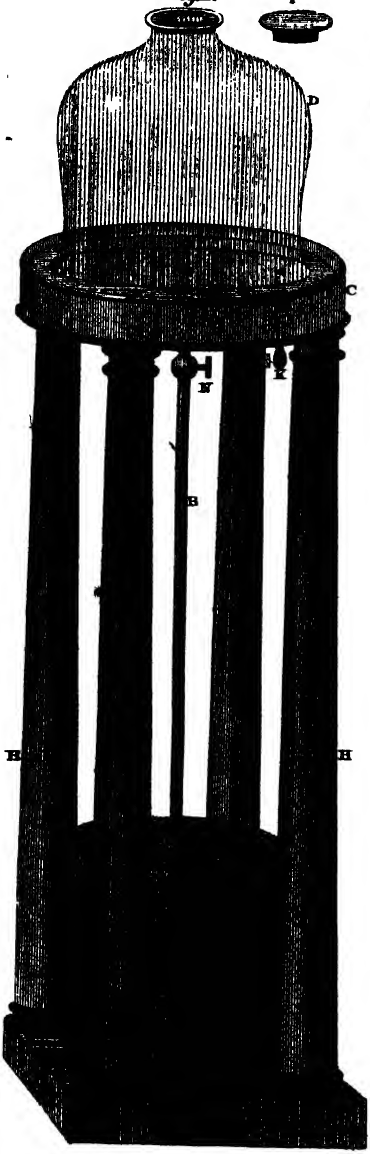
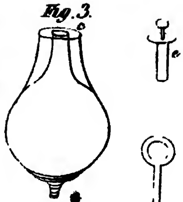


Fig 2.

Fig. 3.



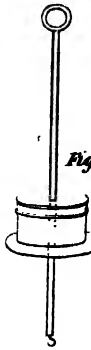


Fig. 4.

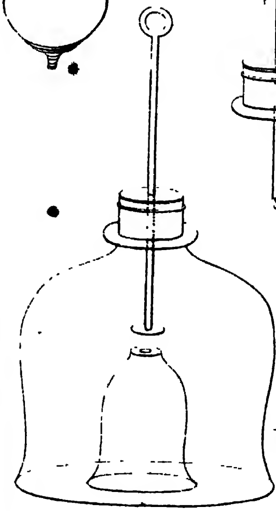


Fig. 5

Problem by J. Gough Esqr.

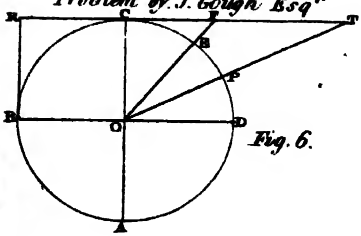


Fig. 6.

X.

Description of an exhausting Machine on the Principle of the Torricellian Vacuum: by Dr. THOMAS STEWART TRAILL.

SOME time ago I was engaged in a series of pneumatic experiments with the air pump, which led me to consider of the best means of obtaining a vacuum. The chief imperfection of an air pump consists in its not being capable of affording a *perfect* vacuum. Each stroke of the piston removes a portion of air in the receiver; but the remaining air expands, until it occupies the same volume which the whole of the included air did. The next stroke abstracts an equal volume of air with the former, but as it is now less dense, the real quantity is smaller; and hence every succeeding stroke removes a less quantity of air than the preceding. The exhaustion goes on, till the elasticity of what remains in the receiver is no longer able to open the valves of the machine, when it has reached the utmost limit. But even if the machine was constructed in the most perfect manner possible, it would evidently be impossible to obtain a complete vacuum on the principle of the air pump: for its effect is expressed by a fraction, the value of which, though constantly increasing, never amounts to unity: *i. e.* though continually approaching to, it never can afford a complete vacuum. Chief defect of an air pump its vacuum being imperfect.

Impressed with these objections to the air-pump, it occurred to me, that, if there was a convenient method of using the Torricellian vacuum, it would be preferable to the common air pump, even when best constructed. After various attempts, the annexed figure and description will give an idea of the machine, which I conceive well adapted to answer the end proposed. Attempts to apply the Torricellian vacuum.

The object in this machine is to procure a vacuum in the receiver D, by means of mercury, with which the receiver is previously filled. A (Pl. I, fig. 1) is a circular plate of thick glass, firmly imbedded in the wooden frame C, which is Apparatus for this purpose described.

is supported by the wooden pillars H. The surface of the plate is to be ground perfectly flat. B is an iron tube, cemented at its upper extremity in a hole drilled in the plate A, and having its lower extremity terminated at the bottom of the wooden tub E, by a stop cock, which is opened and shut by the wire F. The length of the tube ought to be about three feet; its diameter about the size of a common barometer tube. The receiver is to be ground in the usual manner to the plate, and to be fitted with an air-tight cover, I, ground to its upper orifice. K is a stop cock, through which the external air may be admitted when required. A slight depression ought to be made in the glass plate about one inch around G, that the mercury may more readily descend through the tube. The iron tube ought to reach through the glass plate to the bottom of the slight depression; and its inside at the top is to be furnished with a female screw, by which the transferrer, or any other apparatus to be used within the receiver, may be fixed to the plate. It is hardly necessary to observe, that the piece of iron which screws into the upper end of the tube B must be so perforated, as to permit the easy descent of the mercury. The inside of the tub E ought to be coated with strong varnish, to prevent the loss of mercury through its joinings, and may have a cover so fitted to it, as to keep out dust, though not to exclude air. The lower extremity of the tube B ought for steadiness to be fixed to the bottom of the tub, by a flanch and screws. The edges of C ought to project about two inches above the glass plate, that any mercury which falls over may not be lost.

The transferrer, fig. 2, is made like the plate A, and frame C, fig. 1. The lower rim of *a* is intended to rest upon the edges of C, when the iron screw *b* is fixed in G. The key of the stop-cock, *d*, passes through the lower rim, as in the figure.

Method of using the machine.

To use the exhausting machine, draw off the mercury which is in the tub E, by the stop-cock L, leaving just as much as will cover the extremity of the iron tube. Shut the stop-cock at M, pour in mercury at G to fill the tube, anoint the glass plate with hog's lard, place on the receiver, fill the receiver likewise with mercury, and then place its cover

cover I on it. On opening M, the mercury will descend by the tube, and leave the receiver exhausted. By shutting the cock N, the vacuum is rendered more secure.

A small degree of contrivance will adapt this apparatus to every experiment, which can be performed with the common air pump. If we wish to experiment on fluids, they may be enclosed in a flask of the form of fig. 3. The screw at its bottom must be perforated so as to permit the descent of the mercury when it is fixed in G. A ground stopper of glass, *e*, is to be placed in the neck of the flask, *c*, after it is filled with the liquid to be subjected to experiment: the flask is to be screwed to the plate A, and when the receiver is exhausted, the stopper is to be withdrawn by the sliding wire, fig. 4, which with its ground plate is to be substituted for I. The length of the stopper of the flask will afford room for the expansion of the liquid.

Contrivances to adapt it to different experiments, as on fluids.

When we wish to exhibit the pressure of the atmosphere by means of the apparatus, fig. 5, fill both jars, and exhaust them; force down the flat button, which is screwed on the end of the sliding wire, till it covers the orifice of the small jar, and then let the atmospheric air into the outer receiver, by the cock K. The small receiver will adhere to the plate.

Pressure of the atmosphere shown.

By similar slight changes in the other usual apparatus of an air pump, they may be adapted to the exhausting machine.

The advantages of an apparatus, such as I have now described, over the air pump, seem to me of considerable consequence.

Advantages of the apparatus described.

1. The vacuum will be much more perfect; being only affected by the small quantity of air adhering to the mercury, or by the conversion of the mercury into vapour, which is as much as possible obviated by the cock N.

2. There will be a great saving of manual labour.

3. Exhaustion will be more quickly performed than by the common pump.

4. It is more simple than the pump, and less liable to be deranged.

5. The expense of this machine will not exceed, I apprehend, that of one of the best air pumps, while it exhausts more perfectly. Where nice chemical experiments are conducted,

Not expensive.

ducted, a large supply of mercury is indispensable, for there is scarcely a gas, which is not more or less absorbable by water. The mercury of the exhausting engine will answer for the mercurial trough in the laboratory, and thus a considerable portion of the expense of the machine ought to be deducted.

Trial of one rudely executed.

In some experiments I made with a rude machine of the kind I have described, I found, by anointing the plate and edge of the receiver with hog's lard, that I could raise a column of two feet in height in a receiver open at top, and even could move it along on the surface of the ground plate, without any mercury running out between the plate and receiver. In fitting on the top of the receiver, it may however be proper to press gently with the hand on the receiver, till the atmospheric pressure begins to act on it.'

Materials.

N. B. The whole machine, and its auxiliary apparatus, must be made of glass, wood, and iron or steel, on which mercury does not act.

THO. STEWART TRAILL.

Liverpool, May 12, 1808.

Annotation, in Reply to the Doctor's private Letter.

Though mercury has been used for exhaustion by Dr. Clare, and Sir A. N. Edelcrantz, in air pumps described in our Journal, and by others, I have thought Dr. Traill's contrivance sufficiently original, and different from former apparatus, to be inserted.

W. N.

XI.

Doubts respecting some of the received Doctrines of Chance. In a Letter from a Correspondent.

To Mr. NICHOLSON.

SIR, *Durham, August* 9, 1808.

Some received doctrines of

SHOULD the following scruples as to the truth of the elementary doctrines of chance generally admitted appear worthy

worthy of a place in your valuable publication, their insertion will, I hope, elicit from yourself, or some one of your mathematical readers, a few words in reply, to convince or to confute a mind on all occasions suspicious of its own deductions, whenever these deviate from the opinions of others, old and learned in their walks of science.

chance questionable.

The celebrated de Moivre, in his work, Case the first, assumes the die as a familiar and favourable subject for demonstration: let us follow him in sense, though without the advantage of his identical words, as I have not a copy at hand.

Chance of a throw with a single die.

"Any one undertaking, with a die of 6 sides, to cast an ace in one throw, has $\frac{1}{6}$ of the 6 possible chances in his favour, and the remaining $\frac{5}{6}$ against him; the whole 6 chances being certainty, or at least such in the event of continued trials." *Granted*—

Chance of two throws.

"Any one undertaking to cast an ace in two throws of one die, has for the first probability $\frac{1}{6}$, as proved: should the first fail, then the second remains, which is $\frac{1}{6}$ likewise; but the chance of the first failing is $\frac{5}{6}$, as that of its succeeding is $\frac{1}{6}$; therefore the second throw is only $\frac{1}{6}$ of $\frac{5}{6}$ for its chance of success, which added to the chance of casting an ace the first throw, is $\frac{1}{6} + \frac{1}{6}$ of $\frac{5}{6} = \frac{1}{6} + \frac{5}{36} = \frac{6}{36} + \frac{5}{36} = \frac{11}{36}$; the first throw being $\frac{6}{36}$, the second only $\frac{5}{36}$."

2d chance 1 less than 2.

This doctrine I cannot grant. Nothing can prevent him of the second throw, except his succeeding in the first; therefore, either he has no occasion for it, or he has it in all its full force and virtue of $\frac{1}{6}$ chance, from which no circumstance can deduct. Otherwise it must be denied, that two equal chances are twice as good as one; and by summing up, according to de Moivre's rules, the probability of casting an ace in six throws of one die, it will of course be found, that they are below $\frac{6}{6}$, to which they should of course amount, being the assumed sum of certainty on the event, upon an average of trials.

But all the parts must be equal to the whole.

The chance of throwing a head with a halfpenny in two throws, according to de Moivre, is, for the first, $\frac{1}{2}$; for the second, only $\frac{1}{2}$ of $\frac{1}{2}$: so that the one is twice as good as the other, and together they are $\frac{1}{4}$ short in probability of what was assumed certainty, on the amount of all chances.

Instance in the toss of a halfpenny.

Any

Any of your correspondents, or yourself, being kind enough to explain de Moivre to conviction, or my opinion to confutation, will, for I pursue but the truth, equally oblige, Sir,

Your constant reader,

and most obedient servant,

OPSIMATH.

XII.

Letter from a Correspondent on the late Discovery of Metals in the fixed Alkalis.

SIR,

Assertion of Dr Beddoes, that the earths and alkalis contained oxigen.

YOUR last reminds me of some notes I took at Oxford, on attending Dr. Beddoes's lectures in 1788, wherein he said, that vital air was a part of the alkalis and earths. At the same lectures, the strongest electricity was advised, for giving shocks to molten phosphorus, &c. Some years ago, at a friend's, I saw a book with essays by several hands—Dr. Beddoes and Mr. Davy among others. It is called Contributions, &c. I think;—be that as it may, a query is started, whether the earths and alkalis hold not oxigen, and that they may come to class with metals. It is always curious to know who has guessed best. If you have the above book, you may find reasons for opinions then seeming strange, and farther particulars.

Their metallic nature conjectured.

A. DILETTANTE.

8th *Aug*. 1808.

REMARK.

Metals long ago supposed to be obtained from earths.

The notion of alkalis being oxigenated metals capable of being reduced, is much older than the book in question. The experiments of von Ruprecht and Tondi at Schemnitz, with the discussions which arose from them among some of the most eminent chemists, not only in Germany, but in Italy and France, are fresh in remembrance. It is certain, that metals more or less resembling iron, or phosphuret of iron, were produced, in appearance from barytes, lime, magnesia,

nesia, and *borax*; and though this was at length supposed to be refuted, as similar grains of metal were obtained without either of these, yet I believe in this case an alkali was always present. It must be confessed however, the metal produced by the German chemists was extremely different from either of the metallic bases of the alkalis lately discovered by Mr. Davy.

An alkali always present.

XIII.

On the Decomposition of the Alkalis. In a Letter from Mr. William Cooke.

To Mr. NICHOLSON.

SIR, *Wolverhampton*, 10*th July*, 1808.

IN your excellent Journal for January last, under the head Scientific News, is announced the decomposition of the alkalis, by that eminent chemist, Professor Davy; whose very name almost deters one from entertaining a contrary idea: but the conclusions drawn by him do not appear to me to arise from the facts adduced. Since that time I have turned over your Journal, and other publications, in hopes that some one with more leisure and abilities would have pointed out, not the want of accuracy in his experiments, but of clearness in the conclusions drawn therefrom; but not seeing any thing of this kind, I have determined to devote a few minutes from business to offer the following: and if it appear worthy of your notice, it is much at your service, from,

Decomposition of the alkalis questioned.

Sir,

Your most obedient servant,

WILLIAM COOKE.

In Volume XIX, page 78, of your Journal, it appears, that Mr. Davy made *moistened* potash, soda, &c. part of a galvanic circle, in which situation oxigen gas was evolved, and a substance

Moistened alkali placed in a galvanic circle.

a substance of a metallic appearance remained, which was denominated the base of potash, &c., and possessed, among others, the following properties. If a globule were thrown into water, or upon ice, a bright flame and great heat were produced, hidrogen gas was evolved, and the alkali found in solution; if upon moist turmeric paper, the same phenomena appeared, with the acquirement of a rapid motion, and its course marked with a red or brown stain, proving the reproduction of the alkali. In all which instances it is stated, that this metallic body has such an affinity for oxigen, that it instantly decomposes water, absorbing its oxigen (which regenerates alkali) and its hidrogen of course is set at liberty. But, if the experiments with metals be faithfully reported, alkalis are regenerated from these supposed bases, either by losing or absorbing oxigen.

Properties of the substance produced.

Alkali regenerated by losing or absorbing oxigen.

It seems reasonable to conclude, that the matter of electricity is as capable of combining chemically with bodies, as the matter of heat or light is; and that Mr. Davy has found the means of uniting another of the simple combustibles with the alkalis.

Matter of electricity capable of combination.

Perhaps to say *concentrated* sulphuric acid is a calorated oxigat of sulphur may appear barbarous; yet it is impossible to form it without the union of caloric, or to dilute it without the loss thereof: therefore, as I cannot find a more expressive name for these new bodies, I will call them electrated hidrats of potash, soda, &c.; wherein the hidrogen has so weak an affinity for the alkalis, that solution decomposes them; for on coming into contact with water, they are so rapidly decomposed, that the matter of electricity becomes visible, the hidrogen takes the gaseous form, and of course the alkali remains in solution.

Sulphuric acid a compound of caloric.

New bodies, hidrats combined with electricity.

The importance of accounting for the *whole* of the simples submitted to chemical experiment cannot be too often enforced; and that experiments may be depended upon, it is absolutely necessary. The overlooking of this seems to me the only cause of Mr. Davy's miftake: for, as the alkalis were moistened, and it is the known property of the galvanic fluid to decompose water, and as one of its component parts was evolved, it was absolutely necessary to inquire after the other; more especially, as the body produced

In chemical experiments all the component parts should be accounted for.

duced was found to be incompatible with the presence of water.

Thus, if the above arguments be conclusive (but, from my time being necessarily devoted to business, I have not opportunity of submitting them to the rigorous test of experiment, as they deserve, or as I could wish), it appears, that these new substances, instead of being the bases of alkalis, are compounds of alkali and hidrogen united by means of the electric fluid.

XIV.

On the Quantity of Fecula in different Varieties of the Potato. By Mr. WILLIAM SKRIMSHIRE, *Jun.*

SIR, *Wisbech, Aug.* 12, 1808.

IF the following paper, on the quantity of fecula in the different varieties of the *solanum tuberosum*, which was lately read before a small society of philosophical amateurs in this town, be deemed worthy a place in your valuable miscellany, I shall be glad to have it inserted: and shall soon follow this up by a second communication, on the quantity of fecula procured from some other vegetables of British growth, and the economical purposes, to which they may be applied.

Philosophical society at Wisbech.

I remain, yours, &c.

W. SKRIMSHIRE, JUN.

In the early part of the present summer I undertook a series of experiments, to ascertain the quantity of fecula contained in the several varieties of the potato cultivated in this neighbourhood, which I take the liberty of laying before the society, for their information, and as a subject well worthy of a farther investigation.

Quantity of fecula in the potato sought.

But as the following experiments were made with the fresh roots, and at a time of the year when most of them were in a growing state, the several results can be viewed merely

Experiment made with fresh roots.

merely as approximations toward the truth, or at most, as showing the relative quantity of fecula, afforded by the different varieties which were operated upon.

Cautions.

When experiments are made in the large way, with the fresh potato, the different degrees of moisture, which the roots may possess, will materially influence the results; so will the form of the grater, and the force which is employed in grating them. Therefore when great accuracy is required, the potatoes should be sliced, dried, and ground to meal, before being subjected to experiment.

Fecula most abundant in autumn, partly changing to saccharine matter in spring.

Perhaps the greatest quantity of fecula may be procured in the autumn, as soon as the potatoes are dug up; for those that have been preserved through the winter are so disposed to vegetate in the spring, that they then contain more of the saccharine matter than they do in the autumn: and this is produced at the expense of the fecula, for it is probable, that, as the fecula absorbs oxigen and hidrogen, it parts with a portion of its carbon, and is thereby converted into sugar.

Potato attracts moisture from the atmosphere.

Another circumstance, which may very much affect the apparent quantity of fecula, is its precise state of dryness when weighed; for it quickly attracts moisture from the atmosphere, and therefore should always be weighed at a certain temperature, in a dry room.

Fecula dried in a roaster.

In the following experiments, the fecula, after being dried in the open air, was placed for some hours in a Rumford roaster, moderately warm, and weighed as soon as it was taken out. This perhaps is one reason why my produce is generally below that of Dr. Pearson, which he communicated to the Board of Agriculture, as well as by his not using the skin of the potatoes in his experiments, as I did in those which follow.

Skin of the potato included.

Dr. Pearson's experiments with the kidney potato.

From Dr. Pearson's account we learn, that 3500 grains of fresh potato root, commonly called the white kidney, being dried, leave 1000 grains:

That 100 parts of the fresh root, deprived of skin, afford

1. Water..........	68	to 72
2. Meal	32	to 28
	100	100

The

The meal consists of three substances,

1. Starch or fecula	17 to	15
2. Fibrous matter	9 to	8
3. Extract or soluble mucilage	6 to	5
	32 ..	28

Thus 100 parts of fresh root afford from 15 to 17 of fine dry fecula*.

Method in which Mr. Skrimshire's experiments were made.

The following are the results of my experiments made with five pounds of each variety of the potato, weighed after being washed clean, brushed with a hard brush, and wiped dry with a clean linen cloth. The root was afterward grated in cold water. The whole of the pulp and water was placed in a fine hair sieve to drain, and fresh water poured over it, stirring up and squeezing it with the hand, until the water passed through perfectly clear.

By this operation *almost* the whole of the fecula or starch is washed from the fibrous matter, and falls to the bottom of the vessel in the form of a firm white precipitate. This precipitate was again edulcorated with water, and passed through a fine silk sieve, which separated it still more from the finer particles of the pulp. The fecula was then allowed to settle, and being collected was dried by a free exposure to the air, on a clean linen cloth.

1. *Captain Hart.*

Potato called captain Hart.

This is a roundish white potato, with a thin smooth skin, of a moderate size, and with but few eyes. When boiled and peeled it appears of a yellow colour; its consistence is rather close and watery, but it is tolerably well flavoured.

A peck weighs from 14 to 15 lb.

Five pounds weight afford,

	lb.	oz.
Fine fecula very white		9
Fecula slightly discoloured		3
Pulp dried		6
Water, soluble mucilage, and extractive matter	3	14
	5	0

* Repert. Arts, &c. vol. III, p. 383.

2. *Rough*

2. *Rough Red.*

Rough red.

This is a round red potato, of a moderate size, with a thin skin, rough with minute fissures and scales. When boiled it is very mealy, but has rather a strong flavour.

A peck weighs from 13 to 14 lbs.

Five pounds weight afford,

	lb.	oz.
Fine white fecula		7¼
Fecula discoloured		3¼
Pulp dried		6½
Water, soluble mucilage, and extractive matter	3	15
	5	0

3. *White Kidney.*

White kidney.

This is a clean white potato, of a tolerable size, variously shaped, generally flattened, and often with an indentation on one edge, giving it some resemblance to a kidney in its form. The skin is rather thick; when boiled it is not very mealy, but pleasant flavoured, and a very good potato for the table.

A peck weighs from 14 to 15 lbs.

Five pounds weight afford,

	lb.	oz.
Fecula, the whole of which was of an indifferent colour*		9¼
Pulp dried slightly brown†		3¾
Water, soluble mucilage, and extractive matter	4	2¾
	5	0

4. *Moulton White.*

Moulton white.

This is an irregular shaped white potato, of a tolerable size. It is sometimes flattened like the kidney; and in-

* This I attribute to the potatoes being much grown.

† The roaster being too hot, when the pulp was put in, it was rather scorched.

deed

deed its general appearance bears a striking resemblance to the white kidney potato. Its skin is rather thick. When boiled it is very mealy, and remarkably well tasted. It is by far the best potato for the table.

A peck weighs about 16 lbs.

Five pounds weight afford,

	lb.	oz.
Fine fecula very white		9
Fecula slightly discoloured		2¼
Pulp dried		5¼
Water, soluble mucilage, and extractive matter	3	14½
	5	0

5. *Yorkshire Kidney.*

Yorkshire kidney.

This is nothing like the kidney potato, and I think was misnamed. It is a thin, long, white root, with several eyes, is very scabby, and has a thick skin. When boiled it is slightly mealy, but has a strong taste.

A peck weighs from 14 to 15 lbs.

Five pounds weight afford,

	lb.	oz.
Fine fecula very white		8¾
Fecula slightly discoloured		2½
Pulp dried		6¼
Water, soluble mucilage, and extractive matter	3	14½
	5	0

6. *Hundred Eyes.*

Hundred eyes.

This is a long white potato of a midling size. It has numerous eyes, with a narrow transverse depression below each eye. When boiled it has no unpleasant taste, but is rather too close in its consistence to be reckoned a good potato for the table.

A peck weighs from 14 to 15 lbs.

Five

Five pounds weight afford,

	lb.	oz.
Fine white fecula		8¼
Discoloured fecula		¾
Pulp dried		6¾
Water, soluble mucilage, and extractive matter	4	0¼
	5	0

7. *Poor Man's Profit, or Purple Red.*

Poor man's profit, or purple red.

This is a large round purple potato, with a thin skin. When boiled it is hard and close, but has no very unpleasant taste.

A peck weighs from 15 to 16 lbs.

Five pounds weight afford,

	lb.	oz.
Fine fecula very white		8
Very brown fecula		½
Pulp dried		5
Water, soluble mucilage, and extractive matter	4	2½
	5	0

8. *Ox Noble.*

Oxnoble.

This is a very large, round, white potato; but when it grows extremely large, it is frequently found hollow. It is principally used for feeding cattle. When boiled it is close and watery, with a very strong taste.

If perfectly sound, a peck usually weighs from 15 to 16 lbs.

Five pounds weight afford,

	lb.	oz.
Fine white fecula		6¼
Fecula slightly discoloured		1¾
Pulp dried		8
Water, soluble mucilage, and extractive matter	3	15¾
	5	0

XV.

XV.

Account of a Phenomenon, that occurred at Bussora. From Travels in Asia and Africa, by the late ABRAHAM PARSONS, *Esq., Consul and Factor-Marine at Scanderoon.*

MARCH the 15th, 1775. At four this afternoon, the sun then shining bright, a total darkness commenced in an instant, when a dreadful consternation seized every person in the city of Bussora, the people running backward and forward in the streets, tumbling over one another, quite distracted, while those in the houses ran out in amazement, doubting whether it were an eclipse, or the end of the world. Soon after the black cloud which had caused this total darkness, approached near the city, preceded by as loud a noise as I ever heard in the greatest storm. This was succeeded by such a violent whirlwind, mixed with dust, that no man in the streets could stand upon his legs; happy were those who could find, or had already obtained, shelter, while those who were not so fortunate were obliged to throw themselves down on the spot, where they ran great risk of being suffocated, as the wind lasted full twenty minutes, and the total darkness half an hour. The dust was so subtile, and the hurricane so furious, that every room in the British factory was covered with it, notwithstanding we had the precaution to shut the doors and windows on the first appearance of the darkness, and to light candles.

Sudden darkness

attended with a violent whirlwind mixed with dust.

At half past five the cloud had passed the city, the sun instantly shone out, no wind was to be heard, nor dust felt, but all was quite serene and calm again; when all of us in the factory went on the terrace, and observed the cloud had entirely passed over the river, and was then in Persia, where it seemed to cover full thirty miles in breadth on the land, but how far in length could not be even guessed at: it flew along at an amazing rate, yet was half an hour in passing over the city. It came from the north-west, and went straight forward to the south-east.

The cloud 30 miles broad, & half an hour in passing.

The officers of the company's cruizers came on shore as soon as the cloud had passed their ships, and declared that the wind was so violent, and the dust so penetrating, that no man could stand upon the decks; and that after it was over, every place below on board the ship was covered with dust. Such a phenomenon never was known before, in the memory of the oldest man now living at Bussora.

SCIENTIFIC NEWS.

St. Thomas's and Guy's Hospitals.

Medical and surgical lectures.

THE autumnal Course of Lectures at these adjoining Hospitals will commence the beginning of October: viz.

At St. Thomas's.

Anatomy and the Operations of Surgery, by Mr. CLINE and Mr. COOPER.

Principles and Practice of Surgery, by Mr. COOPER.

At Guy's.

Practice of Medicine, by Dr. BABINGTON and Dr. CURRY.

Chemistry, by Dr. BABINGTON, Dr. MARCET, and Mr. ALLEN.

Experimental Philosophy, by Mr. ALLEN.

Theory of Medicine, and Materia Medica, by Dr. CURRY and Dr. CHOLMELEY.

Midwifery, and Diseases of Women and Children, by Dr. HAIGHTON.

Physiology, or Laws of the Animal Œconomy, by Dr. HAIGHTON.

Occasional Clinical Lectures on select Medical Cases, by Dr. BABINGTON, Dr. CURRY, and Dr. MARCET.

Structure and Diseases of the Teeth, by Mr. FOX.

N. B. These several lectures are so arranged, that no two of them interfere in the hours of attendance; and the whole is calculated to form a complete course of Medical and Chirurgical instructions.

London Hospital.

On Monday, the 3d of October, Dr. BUXTON commences a course of Lectures on the Theory and Practice of Medicine, and one on Materia Medica.

Chemical and mineralogical lectures.

Mr. ACCUM's Lectures on Experimental Chemistry and Analytical Mineralogy, commence at the Chemical Laboratory, Compton Street, Soho, October the 18th.

The Lectures on Experimental Chemistry comprise the Practical Operations of the Scientific Laboratory, General Rules to be observed in the performance of Experiments, and Summary Experimental Elucidations of the Science of Chemical Philosophy.

The Lectures on Analytical Mineralogy are devoted to the art of distinguishing minerals, the modes of examining them by chemical agencies and general processes of analysis; with a summary view of the nature of Mineralogical Science, and its application to the useful arts and manufactures.

Account

Account of the Situation of the Instruments employed by Mr. Robert Banks, *for the Meteorological Journal.*

THE barometer, the height of which is registered every day at 9 A. M., is placed at the height of 27 feet from the ground. Now we learn from Dr. Young, that the Thames at Buckingham stairs, 15½ feet below the pavement in the left hand arcade, is 43 feet above the level of the sea, by barometrical comparison with the Seine and the Mediterranean. But he observes, that this calculation probably gives the height too great. Mr. Brindley, levelling from Boulter's Lock to Mortlake, by order of the City of London, in the year 1770, found the fall upon 41 miles to be 75½ feet. On the last 8 miles of this distance, however, the fall was only 12 feet. Now if we allow the fall from Buckingham stairs to the Lower Hope to average only 1 foot per mile, the difference of level will be at least 35 feet. This, added to 42 feet, the height of the ground where Mr. Banks's house stands above the Thames at Buckingham stairs, and 27 feet, the height of the barometer above the ground, we shall have 104 feet for the height of the barometer above the level of the sea.

Barometer.

Calculation of its height above the sea.

104 feet.

The thermometers hang 5 feet from the ground, against a wall that has nearly an eastern aspect, and is completely sheltered from the sun both at its back and front the whole day, in such a manner that it cannot be affected by its heat, either direct or reflected. Five are generally employed for the purpose, because it is well known, if the bulbs be not of the same size, they are subject to vary a little when the temperature is taken at any stated hour, some rising or falling more quickly than others from this circumstance, though a little sooner or later they would indicate the same height. For this reason a mean of them is taken.

Thermometers.

Under the head of weather, if any rain have fallen during the day, the word *rain* is inserted in the day column. The weather in the night column is noted about eleven o'clock, P.M., at which time, if any rain fall, rain is set down; if it do not rain, yet no stars are to be seen, the word *cloudy* is inserted; when there is no rain, and a greater or less number of stars are visible, it is marked as *fair*.

Weather.

METEOROLOGICAL JOURNAL

For AUGUST, 1808,

Kept by ROBERT BANKS, Mathematical Instrument Maker, in the STRAND, LONDON.

JULY. Day of	THERMOMETER. 11 A. M.	11 P. M.	Highest.	Lowest.	BAROMETER.	WEATHER. Night.	Day.
26	66	64	69	61	29·82	Fair	Rain
27	67	63	72	60	29 83	Rain	Fair
28	63	62	66	62	29 52	Cloudy	Rain
29	66	63	70	62	29·68	Fair	Fair
30	68	67	73	61	29·76	Ditto	Ditto
31	70	67	72	62	29·82	Rain*	Rain
AUG.							
1	68	65	75	60	29·64	Fair	Ditto
2	69	65	78	60	29·83	Ditto	Fair
3	61	61	69	60	30·09	Ditto	Ditto
4	67	65	75	60	30·06	Ditto	Ditto
5	68	71	74	64	29 88	Ditto	Ditto
6	69	70	76	62	29·75	Ditto	Ditto
7	68	65	73	61	29·86	Ditto	Rain
8	66	65	72	61	29·82	Ditto	Ditto
9	63	65	71	60	29·64	Ditto †	Ditto ‡
10	62	64	71	62	29·78	Ditto	Fair ‡
11	61	64	70	59	29·74	Ditto	Ditto
12	66	66	73	62	29·82	Ditto	Ditto
13	64	66	68	60	29·73	Cloudy	Rain
14	67	63	72	60	29·70	Fair	Ditto
15	64	63	68	61	29·66	Cloudy	Ditto
16	65	63	69	60	29·87	Fair	Ditto
17	64	64	69	60	29·92	Cloudy	Fair
18	65	64	70	61	30·03	Fair	Ditto
19	64	64	69	58	30·10	Ditto	Ditto
20	64	65	71	58	30·15	Ditto	Ditto
21	65	66	72	62	30·17	Ditto	Ditto
22	65	64	71	58	30·16	Ditto	Ditto
23	64	62	75	59	30·15	Cloudy	Ditto
24	65	62	72	58	30·11	Fair	Ditto
25	63	63	68	56	30·11	Ditto	Ditto
26	64	62	70	59	29·66	Ditto	Ditto

* Hard rain, thunder, and vivid lightning all the evening.

† Thunder at 6 P. M. ‡ Thunder.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

OCTOBER, 1808.

ARTICLE I.

Method of making a Composition for Painting in Imitation of the ancient Grecian Manner, with Remarks. By Mrs. HOOKER, *of Rottingdean, near Brighton**.

SIR,

I Had the pleasure to communicate to the Society for the Encouragement of Arts, Manufactures, and Commerce, in 1786, when Miss E. J. Greenland, my method of painting in imitation of the ancient Grecian manner or encaustic painting; and in consequence, they did me the honour to adjudge to me the gold pallet, and also afterward to approve my account of the result of above fifty experiments per day, which I made during more than four months in 1792, in the hope of discovering some means of making wax, gum mastich, and water unite like a cream, in order to expedite the formation of the composition for imitating the encaustic painting, which was published the same year by the Society

First attempt to imitate the ancient Grecian painting.

* Trans. of the Society of Arts, vol. XXV, p. 45. This lady's first account of her method of painting was published in the 10th vol. of the Trans. of the Society.

of Arts. I now take the liberty of sending them another copy, but with some alterations and many additions, which I trust will be found calculated to facilitate and improve that method of painting, as they have arisen from much observation and reflection on several pictures I have painted since I had last the honour of addressing the Society. In consequence of the application of several gentlemen of the profession, I have drawn up this paper, which, considering the former attentions of the Society, I thought it would be proper for me to offer first to them for their acceptance, but if they should not think it worthy of communication, I hope they will pardon the intrusion, and attribute it only to the sense of gratitude I feel for the honour already conferred on,

Sir,

Your most obedient servant,

EMMA JANE HOOKER.

Method of preparing and applying the composition.

Method of preparing the composition.

Put into a glazed earthen vessel four ounces and a half of gum arabic, and eight ounces, or half a pint (wine measure) of cold spring water; when the gum is dissolved, stir in seven ounces of gum mastich, which has been washed, dried, picked, and beaten fine. Set the earthen vessel containing the gum-water, and gum mastich, over a slow fire, continually stirring and beating them hard with a spoon, in order to dissolve the gum mastich: when sufficiently boiled, it will no longer appear transparent, but will become opaque, and stiff, like a paste. As soon as this is the case, and that the gum-water and mastich are quite boiling, without taking them off the fire, add five ounces of white wax, broken into small pieces, stirring and beating the different ingredients together, till the wax is perfectly melted and has boiled. Then take the composition off the fire, as boiling it longer than necessary would only harden the wax, and prevent its mixing so well afterwards with water. When the composition is taken off the fire, and in the glazed earthen vessel, it should be beaten hard, and

and while hot (but not boiling) mix with it by degrees a pint (wine measure) or sixteen ounces more of cold spring water, then strain the composition, as some dirt will boil out of the gum mastich, and put it into bottles: the composition, if properly made, should be like a cream, and the colours, when mixed with it, as smooth as with oil.

Method of using it.

The method of using it is, to mix with the composition, upon an earthen pallet, such colours in powder as are used in painting with oil; and such a quantity of the composition is to be mixed with the colours, as to render them of the usual consistency of oil colours; then paint with fair water. The colours when mixed with the composition may be laid on either thick or thin, as may best suit your subject; on which account, this composition is very advantageous, where any particular transparency of colouring is required; but in most cases it answers best, if the colours be laid on thick, and they require the same use of the brush, as if painting with body colours, and the same brushes as used in oil painting. The colours, if grown dry, when mixed with the composition, may be used by putting a little fair water over them; but it is less trouble to put some water when the colours are observed to be growing dry. In painting with this composition the colours blend without difficulty when wet, and even when dry the tints may easily be united by means of a brush and a very small quantity of fair water.

The colours may be moistened with water, when grown dry.

Wax to be applied to the painting.

When the painting is finished, put some white wax into a glazed earthen vessel over a slow fire, and when melted, but not boiling, with a hard brush cover the painting with the wax, and when cold take a moderately hot iron, such as is used for ironing linen, and so cold as not to hiss, if touched with any thing wet, and draw it lightly over the wax. The painting will appear as if under a cloud till the wax is perfectly cold, as well as whatever the picture is painted upon; but if, when so, the painting should not appear sufficiently clear, it may be held before the fire, so far from it as to melt the wax but slowly; or the wax may be melted by holding a hot poker at such a distance as to melt it gently, especially such parts of the picture as should not

 appear

appear sufficiently transparent or brilliant; for the oftener heat is applied to the picture, the greater will be the transparency and brilliancy of colouring; but the contrary effect would be produced, if too sudden or too great a degree of heat was applied, or for too long a time, as it would draw the wax too much to the surface, and might likewise crack the paint. Should the coat of wax, put over the painting when finished, appear in any part uneven, it may be remedied by drawing a moderately hot iron over it again as before mentioned, or even by scraping the wax with a knife: and should the wax by too great or too long an application of heat form into bubbles at particular places, by applying a poker heated, or even a tobacco-pipe made hot, the bubbles would subside; or such defects may be removed by drawing any thing hard over the wax, which would close any small cavities. When the picture is cold, rub it with a fine linen cloth.

Wood, canvass, pasteboard, or plaster of Paris may be painted on thus.

Paintings may be executed in this manner upon wood (having first pieces of wood let in behind, across the grain of the wood, to prevent its warping), canvass, card, or plaster of Paris. The plaster of Paris would require no other preparation than mixing some fine plaster of Paris in powder with cold water the thickness of a cream; then put it on a looking-glass, having first made a frame of bees wax on the looking-glass the form and thickness you would wish the plaster of Paris to be of, and when dry take it off, and there will be a very smooth surface to paint upon. Wood and canvass are best covered with some gray tint mixed with the same composition of gum arabic, gum mastich, and wax, and of the same sort of colours as before mentioned, before the design is begun, in order to cover the grain of the wood or the threads of the canvass.

A composition without wax.

Paintings may also be done in the same manner with only gum-water and gum mastich, prepared the same way as the mastich and wax; but instead of putting seven ounces of mastich, and when boiling, adding five ounces of wax, mix twelve ounces of gum mastich with the gum-water, prepared as mentioned in the first part of this receipt: before it is put on the fire, and when sufficiently boiled and beaten, and

a little

a little cold, stir in by degrees twelve ounces, or three quarters of a pint (wine measure) of cold spring water, and afterward strain it.

It would be equally practicable painting with wax alone, dissolved in gum-water in the following manner. Take twelve ounces, or three quarters of a pint (wine measure) of cold spring water, and four ounces and a half of gum arabic, put them into a glazed earthen vessel, and when the gum is dissolved, add eight ounces of white wax. Put the earthen vessel with the gum-water and wax upon a slow fire, and stir them till the wax is dissolved, and has boiled a few minutes: then take them off the fire and throw them into a basin, as by remaining in the hot earthen vessel the wax would become rather hard; beat the gum-water and wax till quite cold. As there is but a small proportion of water in comparison to the quantity of gum and wax, it would be necessary in mixing this composition with the colours, to put also some fair water. Should the composition be so made as to occasion the ingredients to separate in the bottle, it will become equally serviceable, if shaken before used with the colours. A composition without the mastich.

I had lately an opportunity of discovering, that the composition which had remained in a bottle since the year 1792, in which time it had grown dry and become as solid a substance as wax, returned to a creamlike consistence, and became again in as proper a state to mix with colours, as when it was first made, by putting a little cold water upon it, and suffering it to remain on a short time. I also lately found some of the mixture composed of only gum arabic water and gum mastich, of which I sent a specimen to the Society of Arts in 1792; it was become dry, and had much the appearance and consistency of horn. I found, on letting some cold water remain over it, that it became as fit for painting with, as when the composition was first prepared. The composition grows dry by keeping, but may be softened afresh with water.

II.

On Oxalic Acid. *By* THOMAS THOMSON, *M. D. F. R. S. Ed.* *Communicated by* CHARLES HATCHETT, *Esq. F. R. S.*

(*Concluded from p. 32.*)

IV. *Composition of Oxalic Acid.*

Composition of oxalic acid.

THE knowledge of the relative weights of the elements which compose oxalic acid, though of importance, is not sufficient to convey a clear idea of this compound, and in what respect it differs from tartaric acid, alcohol, sugar, and various other bodies possessing very different properties, though composed of the very same elements in different proportions.

Elements always combine in determinate proportions which may be expressed by numbers.

It has been ascertained, by numerous and decisive experiments, that elementary bodies always enter into combinations in determinate proportions, which may be represented by numbers. For example, the numbers which correspond to the four elements, oxigen, azote, carbon, and hidrogen, are the following;

Instance.

Oxigen	6
Azote	15
Carbon	4·5
Hidrogen	1

Now, in all compounds consisting of these ingredients, the proportion of the different constituents may always be represented by these numbers, or by multiples of them; thus, the composition of the following substances may be thus stated.

Compounds of oxigen, nitrogen, carbon, and hidrogen.

	Oxigen.	Hidrogen	Carbon.	Azote.
Water	6	+1		
Carbonic oxide	6		+4·5	
Carbonic acid	2×6		+4·5	
Carburetted hidrogen		2×1	+4·5	
Olefiant gas		1	+4·5	
Nitrous gas	6			+5
Nitric acid	2×6			+5
Nitrous oxide	6			+2×5

From

From the knowledge of this curious law, it is difficult to avoid concluding, that each of these elements consists of atoms of determinate weight, which combine according to certain fixed proportions, and that the numbers above given represent the relative weights of these atoms respectively. Thus, an atom of oxigen weighs six, an atom of hidrogen one, &c. Water is composed of one atom of oxigen, and one atom of hidrogen; carbonic acid of two atoms of oxigen, and one of carbon, &c. This curious theory, which promises to throw an unexpected light on the obscurest parts of chemistry, belongs to Mr. Dalton. I have elsewhere illustrated it at considerable length*.

These numbers probably represent the weight of their primitive atoms.

The same law holds with respect to the salts. The acids and bases always combine in determinate proportions. We may affix numbers to all the acids and bases, which numbers, or their multiples, will represent all the combinations into which these bodies enter. Some of these numbers are given in the following table:

The same law holds in salts.

Sulphuric acid	33	Barytes	67
Muriatic acid	18	Soda	24
Carbonic acid	16·5	Lime	23
Nitric acid	17	Ammonia	6

These numbers may be conceived to represent the relative weights of an integrant particle of each of these substances; formed on the supposition, that an atom of hidrogen weighs 1. It follows equally from this law, that the acids and bases combine particle with particle, or a certain determinate number of particles of the one with a particle of the other.

Corollary.

One of the most important points in the investigation of compound bodies is, to ascertain the number, which denotes the weight of an integrant particle of each of them, that of an atom of hidrogen being one; because this number, or a multiple of it, represents the weight of each, which enters into all combinations; and because it enables us to estimate the number of elementary atoms, of which each is composed. From a careful comparison of the table of oxalates, given in a preceding part of this paper, with the weight of the dif-

Weight of integrant particles a object of importance.

* See System of Chemistry, III, 424, &c. 3d Edition.

ferent

ferent bases already determined*, it appears, that the weight of an integrant particle of oxalic acid must be represented by the number 39·5.

Integrant particle of oxalic acid.

Now, what number of atoms of oxigen, carbon, and hidrogen, go to constitute an integrant particle of oxalic acid? We have assigned the relative weights of each of these atoms, and we have ascertained the relative proportions of the respective elements of oxalic acid. From these data it is easy to solve the problem. An integrant particle of oxalic acid consists of 9 atoms combined together, namely, 4 atoms of oxigen, 3 of carbon, and 2 of hidrogen.

4 atoms of oxigen weigh	4×6	$= 24$
3 atoms of carbon	$3 \times 4{\cdot}5$	$= 13{\cdot}5$
2 atoms of hidrogen	2×1	$= 2$
Total		39·5

which together make up the weight of an integrant particle of oxalic acid.

Component parts of the acid according to this.

According to these proportions, 100 parts of oxalic acid is composed of

Oxigen	61
Carbon	34
Hidrogen	5
	100

numbers which do not indeed exactly correspond with the result of the preceding analysis, but which approach sufficiently near it, to give the reasoning employed considerable probability at least, if it does not lead to certainty.

Decmoposition of oxalate of lime by heat explained.

We may now examine the decomposition which takes place, when oxalate of lime is exposed to heat. Let an atom of oxigen be w, an atom of carbon c, and an atom of hidrogen, h. An integrant particle of oxalic acid may be represented by $4\,w + 3\,c + 2\,h$. We may represent the composition and weight of an integrant particle of each of the substances into which oxalic acid is decomposed by heat, by the following symbols and numbers:

* For these weights, and the method of determining them, I refer the reader to my System of Chemistry, 2d Edition, III, 619. The numbers which I have there assigned are, I am persuaded, rather too low.

Carbonic

Carbonic acid	$2w + c$	weight	16·5
Carburetted hidrogen	$c + 2h$		6·5
Carbonic oxide	$w + c$		10·5
Water	$w + h$		7
Charcoal	c		4·5

We may now conceive 3 particles of oxalic acid to be decomposed at once, and to resolve themselves into these substances, in the following proportions:

4 particles of carbonic acid	$= 8w + 4c$
2 particles of carburetted hidrogen	$= 2c + 4h$
2 particles of carbonic oxide	$= 2w + 2c$
2 particles of water	$= 2w + 2h$
1 particle of charcoal	$= 1c$
Total	$12w + 9c + 6h$
3 particles of oxalic acid	$= 12w + 9c + 6h$

We see that such a decomposition is possible. It remains only therefore to see, whether the weights of these substances, which result from this hypothesis, correspond with the preceding analysis. Now,

4 particles of carbonic acid weigh	$4 \times 16·5 = 66$
2 ——— carburetted hidrogen	$2 \times 6·5 = 13$
2 ——— carbonic acid	$2 \times 10·5 = 21$
2 ——— water	$2 \times 7 = 14$
1 ——— charcoal	$4·5 = 4·5$
Total	118·5

Reducing these proportions to 100 parts of acid, and joining together the two inflammable gasses, the numbers come out as follows:

		we actually obtained
Carbonic acid	55·70	59·53
Inflammable air	28·69	24·28
Water	11·81	11·51
Charcoal	3·80	4·68
	100·00	100·00

It is impossible to expect exact correspondence between the theory and analysis, till the numbers representing the weights

Hypothesis and facts nearly agree.

weights of the elementary atoms be ascertained with more rigid accuracy, than has hitherto been done. I satisfied myself with taking the nearest round numbers, which are sufficient at least to show an evident approximation to the proportions obtained by experiment.

V. *Composition of Sugar, and Formation of Oxalic Acid.*

Composition of sugar and formation of oxalic acid.

When a compound body is decomposed, and resolved into a number of new substances, the products are almost always simpler, or consist of integrant particles composed of fewer atoms than the integrant particles of the original body. Thus, though oxalic acid is composed of 9 atoms, none of the products evolved, when that acid is decomposed by heat, contain more than 3 atoms. Hence it is probable, that sugar is a more compound body than oxalic acid, because nitric acid resolves it into a variety of new compounds, one of which is oxalic acid. It may be worth while to examine the action of nitric acid on sugar, and the formation of oxalic acid, more closely than has hitherto been done, as the investigation will furnish some data for estimating the composition of sugar,

200 grains of sugar treated with nitric acid.

Two hundred grains of pure crystallized sugar, being treated with diluted nitric acid in the usual way, yielded 200 cubic inches of carbonic acid, 64 cubic inches of nitrous gas, and 70 cubic inches of azotic gas. But these numbers, though the result of a good many experiments, are not to be considered as very exact. The uncertainty depends upon the property, which the solution has of producing more gas after the sugar is decomposed, at the expense of the oxalic acid formed. Now it is difficult to stop at the precise point.

116 grains of oxalic acid produced.

The whole weight of oxalic acid, which can be obtained from 200 grains of sugar, amounts to 116 grains. If the experiment be properly conducted, the whole of the sugar is decomposed, or at least the quantity of residuary matter is small.

From the preceding statement, there is reason to conclude, that 100 grains of sugar, when decomposed by nitric acid, yield,

1. Oxalic

		Grains.
1.	Oxalic acid crystals 58 grains, or real acid	45
2.	Carbonic acid 100 cubic inches, equivalent to	46·5:

Oxalic and carbonic acids.

while there are evolved obviously, by the decomposition of the nitric acid,

		Grains.
1.	Azotic gas 35 cubic inches, equivalent to	10·62
2.	Nitrous gas 32 cubic inches, equivalent to	10·85

Now, as nitric acid contains no carbon, it is obvious, that the oxalic acid formed, and the carbonic acid evolved, must contain the whole carbon contained in 100 grains of sugar.

	Grains.
45 grains of oxalic acid contain of carbon	14·40
46·5 grains of carbonic acid contain of ditto	13·02
Total	27·42

therefore 100 grains of sugar contain $27\frac{1}{2}$ grains of carbon.

100 sugar contain 27·5 carbon.

The azotic gas and nitrous gas must have been originally in the state of nitric acid, and must have given out oxigen when they were evolved. But nitric acid is composed of

			Oxigen.
Azote	10·62	+	25
Nitrous gas	10·85	+	4·5
			29·5

Therefore they must have parted with 29·5 grains of oxigen. We are at liberty to suppose, that the whole of this oxigen went to the formation of carbonic acid. Now, 46·5 grains of carbonic acid are composed of

	Grains.
Oxigen	33·5
Carbon	13·0
	46·5

From this it appears, that in the carbonic acid there were 4 grains of oxigen more than was furnished by the nitric acid. I confess I am disposed to ascribe this surplus to errours in the experiments, and to believe, that the whole of the oxigen of the carbonic acid was furnished by the nitric acid.

Apparent surplus of oxigen.

acid. This being admitted, it follows, that the carbon of the carbonic acid, and the whole constituents of the oxalic acid, were furnished by the sugar. These are as follows:

	Grains.
Carbon	27·5
Oxigen in 45 grains oxalic acid	28·8
Hidrogen in ditto	1·8
	58·1

Water presumed to be formed.

If this total be substracted from the 100 grains of sugar used, there will be a remainder of 41·9 grains. As this quantity of the sugar has disappeared, and is no where to be found among the products, we must suppose, that it has assumed the form of water. Now 41·9 grains of water are composed of

Oxigen	35·9
Hidrogen	6
	41·9

Adding these quantities to the preceding products, we obtain the composition of sugar, as follows:

Component parts of sugar.

Oxigen	64·7
Carbon	27·5
Hidrogen	7·8
	100·0

This cannot be relied on implicitly,

but corroborated by Lavoisier's analysis.

Though the process of reasoning, which led to this analysis of sugar, is too hypothetical to be trusted implicitly, yet I am persuaded, that it is to a certain degree correct, and that the result obtained does not deviate very far from the truth. If we compare Lavoisier's statement of the composition of sugar obtained in a different manner, though by a mode of reasoning not less hypothetical, we shall be surprised at its near coincidence with mine. His numbers are

Oxigen	64
Carbon	28
Hidrogen	8
	100

It

It is true, that two different hypotheses may lead to the same result, and yet be both wrong; but this becomes infinitely improbable in the present case, when we consider, that the proportion of carbon, which I assign to sugar, must at all events be nearly correct.

Farther argument.

We have no direct method of determining the weight of an integrant particle of sugar; but if the accuracy of the preceding analysis be admitted, it furnishes us with an indirect one, which cannot be rejected; for it is clear, that the atoms of oxigen, carbon, and hidrogen, will be to each other respectively, as the numbers $\frac{64}{6}$, $\frac{28}{4.5}$, $\frac{8}{1}$; and these numbers reduced to their lowest terms become 5, 3, 4, nearly, which, being primes with respect to each other, must represent the number of atoms, of which an integrant particle of sugar is composed.

Integrant particle of sugar.

Sugar then is a compound of 12 atoms; namely, five of oxigen, three of carbon, and four of hidrogen; the weight of an integrant particle of it is 47·5, and its symbol is $5\,w + 3\,c + 4\,h$.

Its difference from oxalic acid.

It differs from oxalic acid merely in containing an additional atom of oxigen and two of hidrogen. If we had any method of removing these substances, without altering the proportion of the other constituents, we should obtain a much greater quantity of oxalic acid from sugar than we can at present; but nitric acid acts by removing one half of the carbon in the form of carbonic acid; the sugar, deprived of this, resolves itself into oxalic acid and water.

Theory of the formation of oxalic acid from sugar.

Suppose two particles of sugar acted on at once, the symbol for them will be $10\,w + 6\,c + 8\,h$. Let three atoms of the carbon be removed by the action of the nitric acid, there will remain $10\,w + 3\,c + 8\,h$. Now

$$\begin{array}{lcl} \text{A particle of oxalic acid} & = & 4\,w + 3\,c + 2\,h \\ \text{Six particles of water} \cdots & = & 6\,w + \quad + 6\,h \\ \hline & & 10\,w + 3\,c + 8\,h, \end{array}$$

which is just the quantity of oxalic acid left. This will give us some idea of the way in which the formation of oxalic acid by nitric acid is accomplished. And although the series of changes is probably more complicated, yet they are ultimately equivalent to the preceding statement.

Malic acid.

I allude to the formation of malic acid, which is said to precede

cede the oxalic acid, and afterward to be converted into it by the subsequent action of nitric acid; but on the composition and formation of this latter acid I avoid making any observations at present, as I propose to make them the subject of a separate dissertation.

III.

An Account of the Application of the Gas from Coal to economical Purposes. By Mr. WILLIAM MURDOCH. *Communicated by the Right Hon. Sir* JOSEPH BANKS, *Bart. K. B. P. R. S.**

Light from coal gas applied on a large scale.

THE facts and results intended to be communicated in this Paper are founded upon observations made, during the present winter, at the cotton manufactory of Messrs. Phillips and Lee at Manchester, where the light obtained by the combustion of the gas from coal is used upon a very large scale; the apparatus for its production and application having been prepared by me at the works of Messrs. Boulton, Watt, and Co. at Soho.

Quantity of light required on a comparison with candles.

The whole of the rooms of this cotton mill, which is, I believe, the most extensive in the United Kingdom, as well as its counting-houses and store-rooms, and the adjacent dwelling-house of Mr. Lee, are lighted with the gas from coal. The total quantity of light used during the hours of burning, has been ascertained, by a comparison of shadows, to be about equal to the light which 2500 mould candles of six in the pound would give; each of the candles, with which the comparison was made consuming at the rate of 4-10ths of an ounce (175 grains) of tallow per hour.

Experiment made under very favourable circumstances.

The quantity of light is necessarily liable to some variation, from the difficulty of adjusting all the flames, so as to be perfectly equal at all times; but the admirable precision and exactness, with which the business of this mill is conducted, afforded as excellent an opportunity of making

* Philos. Trans. for 1808, p. 124.

the

the comparative trials I had in view, as is perhaps likely to be ever obtained in general practice. And the experiments being made upon so large a scale, and for a considerable period of time, may, I think, be assumed as a sufficiently accurate standard for determining the advantages to be expected from the use of the gas lights under favourable circumstances.

It is not my intention, in the present paper, to enter into a particular description of the apparatus employed for producing the gas: but I may observe generally, that the coal is distilled in large iron retorts, which during the winter season are kept constantly at work, except during the intervals of charging; and that the gas, as it rises from them, is conveyed by iron pipes into large reservoirs, or gasometers, where it is washed and purified, previous to its being conveyed through other pipes, called mains, to the mill. These mains branch off into a variety of ramifications (forming a total length of several miles), and diminish in size, as the quantity of gas required to be passed through them becomes less. The burners, where the gas is consumed, are connected with the above mains, by short tubes, each of which is furnished with a cock to regulate the admission of the gas to each burner, and to shut it totally off when requisite. This latter operation may likewise be instantaneously performed, throughout the whole of the burners in each room, by turning a cock, with which each main is provided, near its entrance into the room. Method in which the gas is obtained and applied.

The burners are of two kinds: the one is upon the principle of the Argand lamp, and resembles it in appearance; the other is a small curved tube with a conical end, having three circular apertures or perforations, of about a thirtieth of an inch in diameter, one at the point of the cone, and two lateral ones, through which the gas issues, forming three divergent jets of flame, somewhat like a fleur-de-lis. The shape and general appearance of this tube, has procured it among the workmen the name of the cockspur burner. Two kinds of burners.

The number of burners employed in all the buildings amounts to 271 Argands, and 633 cockspurs; each of the former giving a light equal to that of four candles of the description 904 burners.

description abovementioned; and each of the latter, a light equal to two and a quarter of the same candles; making therefore the total of the gas light a little more than equal to that of 2500 candles. When thus regulated, the whole of the above burners require an *hourly* supply of 1250 cubic feet of the gas produced from cannel coal; the superior quality and quantity of the gas produced from that material having given it a decided preference in this situation, over every other coal, notwithstanding its higher price.

giving light equal to 2500 mould candles of 6 to the lb. consume 1250 cub. feet of gas from cannel coal hourly.

The time during which the gas light is used may, upon an average of the whole year, be stated at least at two hours per day of twenty-four hours. In some mills, where there is over work, it will be three hours; and in the few where night work is still continued, nearly twelve hours. But taking two hours per day as the common average throughout the year, the consumption in Messrs. Philips and Lee's mill, will be $1250 \times 2 = 2506$ cubic feet of gas per day; to produce which, seven hundred weight of cannel coal is required in the retort. The price of the best Wigan cannel (the sort used) is $13\frac{1}{2}d.$ per cwt. (22*s.* 6*d.* per ton), delivered at the mill, or say about eight shillings for the seven hundred weight. Multiplying by the number of working days in the year (313), the annual consumption of cannel will be 110 tons, and its cost £125.

This requires $3\frac{1}{2}$cwt. of cannel coal in the retorts,

About one third of the above quantity, or say forty tons of good common coal, value ten shillings per ton, is required for fuel to heat the retorts; the annual amount of which is £20.

and above 1-3d as much good common coal to heat them.

The 110 tons of cannel coal, when distilled, produce about 70 tons of good coak, which is sold upon the spot at 1*s.* 4*d.* per cwt. and will therefore amount annually to the sum of £93.

Produce in coak.

The quantity of tar produced from each ton of cannel coal is from eleven to twelve ale gallons, making a total annual produce of about 1250 ale gallons, which not having been yet sold, I cannot determine its value; but whenever it comes to be manufactured in large quantities, it cannot be such as materially to influence the economical statement, unless indeed new applications of it should be discovered.

Tar.

Produce trifling.

The

The quantity of aqueous fluid, that came over in the course of the observations which I am now giving an account of, was not exactly ascertained, from some springs having got into the reservoir; and as it has not been yet applied to any useful purpose, I may omit further notice of it in this statement. Aqueous fluid.

The interest of the capital expended in the necessary apparatus and buildings, together with what is considered as an ample allowance for wear and tear, is stated by Mr. Lee at about £550 per annum: in which some allowance is made for this apparatus being upon a scale adequate to the supply of a still greater quantity of light, than he has occasion to make use of. Interest of capital.

He is of opinion, that the cost of attendance upon candles would be as much, if not more, than upon the gas apparatus; so that in forming the comparison, nothing need be stated upon that score, on either side. Attendance not more than on candles.

The economical statement for one year then stands thus: Expense of the gas lights.

Cost of 110 tons of cannel coal	£125
Ditto of 40 tons of common ditto	20
	145
Deduct the value of 70 tons of coak	93
The annual expenditure in coal, after deducting the value of the coak, and without allowing any thing for the tar, is therefore	52
And the interest of capital, and wear and tear of apparatus	550

making the total expense of the gas apparatus, about £600 per annum.

That of candles, to give the same light, would be about £2000. For each candle consuming at the rate of 4 tenths of an ounce of tallow per hour, the 2500 candles, burning upon an average of the year two hours per day, would, at one shilling per pound, the present price, amount to nearly the sum of money abovementioned. That of candles.

If the comparison were made upon an average of three hours per day, the advantage would be still more in favour of the gas light; the interest of the capital, and wear and tear The comparison more favourable to the gas where the

light is continued longer.

tear of the apparatus continuing nearly the same as in the former case; thus,

1250 × 3 = 3750 cubic feet of gas per day, which would be produced by 10¼cwt. of cannel coals; this, multiplied by the number of working days, gives 168 tons per annum, which, valued as before, amounts to £188

And 60 tons common coal for burning under the retorts will amount to..............	30
	218
Deduct 105 tons of coak at 26*s.* 8*d.*	140
Leaving the expenditure in coal, after deduction of the coak, and without allowance for the tar, at	78

Adding to which the interest and wear and tear of apparatus, as before, the total annual cost will not be more than £650, while that of tallow, rated as before, will be £3000.

But an increased expense of apparatus required.

It will readily occur, that the greater number of hours the gas is burnt, the greater will be its comparative economy; although in extending it beyond three hours, an increase of some parts of the apparatus would be necessary.

Advantage above oil less.

If the economical comparison were made with oils, the advantages would be less than with tallow.

Beginning of the experiment.

The introduction of this species of light into the establishment of Messrs. Philips and Lee has been gradual; beginning in the year 1805, with two rooms of the mill, the counting-houses, and Mr. Lee's dwelling-house. After which, it was extended through the whole manufactory, as expeditiously as the apparatus could be prepared.

Inconvenience at first from the smell.

At first, some inconvenience was experienced from the smell of the unconsumed, or imperfectly purified gas, which may in a great measure be attributed to the introduction of successive improvements in the construction of the apparatus, as the work proceeded. But since its completion, and since the persons, to whose care it is confided, have become familiar with its management, this inconvenience has been obviated; not only in the mill, but also in Mr. Lee's house, which is most brilliantly illuminated with it, to the exclusion of every other species of artificial light.

The

The peculiar softness and clearness of this light, with its almost unvarying intensity, have brought it into great favour with the work people. And its being free from the inconvenience and danger, resulting from the sparks and frequent snuffing of candles, is a circumstance of material importance, as tending to diminish the hazard of fire, to which cotton mills are known to be much exposed. Its advantages in a manufactory.

The above particulars, it is conceived, contain such information, as may tend to illustrate the general advantages attending the use of the gas light; but nevertheless the Royal Society may perhaps not deem it uninteresting, to be apprised of the circumstances, which originally gave rise in my mind to its application, as an economical substitute for oils and tallow. Origin of the idea of its application.

It is now nearly sixteen years, since, in a course of experiments I was making at Redruth in Cornwall, upon the quantities and qualities of the gasses produced by distillation from different mineral and vegetable substances, I was induced by some observations I had previously made upon the burning of coal, to try the combustible property of the gasses produced from it, as well as from peat, wood, and other inflammable substances. And being struck with the great quantities of gas which they afforded, as well as with the brilliancy of the light, and the facility of its production, I instituted several experiments, with a view of ascertaining the cost, at which it might be obtained, compared with that of equal quantities of light yielded by oils and tallow. In 1792.

My apparatus consisted of an iron retort, with tinned copper and iron tubes, through which the gas was conducted to a considerable distance; and there, as well as at intermediate points, was burned through apertures of varied forms and dimensions. The experiments were made upon coal of different qualities, which I procured from distant parts of the kingdom, for the purpose of ascertaining which would give the most economical results. The gas was also washed with water, and other means were employed to purify it. First experiments.

In the year 1798, I removed from Cornwall to Messrs. Boulton, Watt, and Co's. works for the manufactory of steam engines at the Soho foundry; and there I constructed Practically applied in 1798.

an apparatus upon a larger scale, which during many successive nights was applied to the lighting of their principal building, and various new methods were practised of washing and purifying the gas.

Illumination in 1802.

These experiments were continued with some interruptions, until the peace of 1802, when a public display of this light was made by me in the illumination of Mr. Boulton's manufactory at Soho, upon that occasion.

Since regularly used at the Soho foundry.

Since that period, I have, under the sanction of Messrs. Boulton, Watt, and Co. extended the apparatus at Soho foundry, so as to give light to all the principal shops, where it is in regular use, to the exclusion of other artificial light; but I have preferred giving the results from Messrs. Philips and Lee's apparatus, both on account of its greater extent, and the greater uniformity of the lights, which rendered the comparison with candles less difficult.

The inflammable nature of the gas long known,

At the time I commenced my experiments, I was certainly unacquainted with the circumstance of the gas from coal having been observed by others to be capable of combustion; but I am since informed, that the current of gas escaping from Lord Dundonald's tar ovens had been frequently fired; and I find that Dr. Clayton, in a paper in volume XLI of the Transactions of the Royal Society, so long ago as the year 1739, gave an account of some observations and experiments made by him, which clearly manifest his knowledge of the inflammable property of the gas, which he denominates "the spirit of coals;" but the idea of applying it as an economical substitute for oils and tallow does not appear to have occurred to this gentleman, and I believe I may, without presuming too much, claim both the first idea of applying, and the first actual application of this gas to economical purposes.

but first applied to economical purposes by Mr. Murdoch.

REMARK.

Intention of applying coal gas to light the streets of the metropolis.

As an attempt to light the streets of the metropolis by means of coal gas has made much noise, and is meant, as it is said, to be brought before parliament next session, whatever tends to elucidate the subject cannot be uninteresting. The account here given by Mr. Murdoch, whose experiments

experiments were mentioned in our Journal for June 1805, is perfectly satisfactory with respect to the application of coal gas as the material of furnishing light, and its comparative cheapness and advantages, at least in a coal country: but it must be obvious, that his calculations are by no means adapted to London.

Mr Murdoch's estimate not applicable to London.

Mr. Murdoch states the annual expense for lighting the manufactory of Messrs. Philips and Lee at £600; and observes, that, to produce an equal light by candles would cost £2000. The cannel coal employed however, as being most profitable though sold at the highest price, costs there only 22*s.* 6*d.* per ton, and the coal for heating the retorts only 10*s.* per ton. The coak produced there sells at 26*s.* 8*d.* per ton. Now on inquiry at a very respectable coal merchant's in London I find, that cannel coal sells here at £4 per ton; the coal for the furnaces may be averaged at 45*s.*, and the coak at 50*s.* It must be observed too, that the apparatus being manufactured on the spot at Birmingham, it of course was so much the less expensive. The statement for the metropolis therefore would probably stand thus.

Calculation for the metropolis for two hours:

Cost of 110 tons of cannel coal at £4	£440
——— 40 tons of common coal at 45*s.*........	90
Interest of capital, and wear and tear of apparatus	650
	1180
Deduct for 70 tons of coak at 50*s.*.............	175
	1005

Thus the expenditure would be £1005 to procure light equal to that of as many candles as would come to £2000. This is the calculation for light for two hours a day. If we take it for three hours a day, according to Mr. Murdoch's second estimate, the calculation will be

or if extended to three.

168 tons of cannel coal	£672
60 tons of common coal	135
Apparatus as before	650
	1457
Deduct for 105 tons of coak	263
	1194

or £1194 to procure the light of £3000 worth of candles.

When

For a longer time more favourable.

When the gas is applied to lighting the streets by night however, perhaps we may state the time at an average as ten hours a day; and the longer the time, as Mr. Murdoch justly observes, the greater will be the balance in favour of the gas, since the apparatus will remain nearly the same.

But various deductions must be made.

But then, there are several circumstances farther to be taken into consideration. Though the greater part of the apparatus would not be altered, it appears an increase of some parts would be necessary, if the burning were to be continued beyond three hours. We must add too to the expenditure, the rent of houses in every part of the town for holding the furnaces and apparatus, the wages of persons to attend these, and the salaries of clerks, none of which were necessary in Mr. Murdoch's case. Besides, the price of coal must probably be increased by the additional consumption, and that of coak would certainly fall very greatly, from the quantity produced beyond the demand for it. The estimate too must be made in comparison with common lamp oil, the expense of which may be reckoned at not more than two thirds the cost of candles. Farther, the gas lights are said to give double the light of the common street lamps, and this is certainly an accommodation to the public: but then, as the calculation of Mr. Murdoch is founded very properly on the quantity of light given, this will affect the estimate in a similar ratio, so that the expense of the oil must be diminished by one half. On these grounds the estimate would appear somewhat in the following form, taking it at an average of ten hours every night.

Estimate without these deductions.

550 tons of cannel coal	£2200
200 tons of common coal	450
Interest of capital, and wear and tear of apparatus	800
	3450
Deduct for 350 tons of coak	875
	2575.

The expense of lighting to an equal extent with oil, according to the estimation above given, would be £3333. It must be observed, nothing is here allowed for the coal-tar, the

the produce of which from the above quantity of coal,* according to Mr. Murdoch, would be about 6000 ale gallons, or 7000 wine gallons; as he has left it out of his estimate from being of too trifling value. Neither have I taken into account the expense for rents of houses, salaries, and wages, with the other circumstances abovementioned, that must affect the profits; though I have said enough to show, that these, if any, must differ very widely indeed from the enormous gains held out to the public, to induce incautious individuals to embark in the project, when it was first set on foot.

For farther remarks on the coal gas lights see our Journal, vol. XVI, p. 73, 83, 308. C.

IV.

Farther Experiments on the Spleen. By EVERARD HOME, *Esq. F. R. S.**

THE results of the experiments already brought forward† having established the fact, that fluids received into the stomach, when the pylorus is closed, pass through the spleen into the circulation of the blood; it became an object to determine, by experiment, whether this takes place, when the parts are in a natural state. Object in view.

The ass appeared, on many accounts, the best subject for this purpose; and as it is made use of to teach the veterinary pupils the anatomy of that tribe of animals, I applied to the Professor for permission to make my experiments in the theatre of the college. The ass a favourable subject.

This was granted me in the most obliging manner; the subjects were also supplied by the College, and Mr. Sewell, the assistant Professor, gave me his personal aid with a degree of zeal and ability I have rarely met with, and have much pleasure in acknowledging.

* Philos. Trans. for 1808, p. 133.

† See our Journal, vol. XX, p. 374.

In

In making the following experiments, I had the assistance of Mr. Sewell, Mr. Brodie, Mr. William Brande, and Mr. Clift.

Exp. 1. Tincture of rhubarb given diluted with water.

Experiment 1. An ass, which had been kept twenty-four hours without hay, to prevent the liquor that was to be poured into its stomach from being soaked up and retained there, on the evening of the 3d of December, 1807, had a drench given it, consisting of half a pint of the spirituous tincture of rhubarb, diluted in half a pint of water. On the morning of the 4th, this was repeated at eight o'clock, and again at twelve. At two o'clock the animal was pithed, so as to destroy its sensibility; and before the circulation was entirely stopped, six ounces of blood were taken from the splenic vein into a graduated glass measure, and a similar quantity was taken from the left auricle of the heart, into a vessel of the same kind: these were allowed to coagulate and separate their serum.

The animal pithed. Blood drawn from the splenic vein and left auricle.

State of the spleen.

The spleen was large and turgid; upon making sections of it, the cells were found to be very numerous; and towards the great end and near the edge, they were particularly distinct to the naked eye. The cut surface had a strong smell of rhubarb, and when it was applied to white paper wetted with the alkaline test, an orange tinge was produced. This was strongly contrasted by a stain made in the same manner with a section of the liver, which had no such tinge, nor did the liver give the slightest smell of rhubarb.

Rhubarb conveyed to it, but not to the liver.

Rhubarb in the urine most: next in infusion of spleen, then in serum from splenic vein, and least in serum from left auricle and infusion of liver.

Infusions were made of the spleen and liver under similar circumstances; these were strained off into separate glasses, and tested by the alkali. The urine was tested in the same way. The serum, from the different portions of blood, was also poured off into separate glass vessels, to which the test was added. In nineteen hours after the blood had been taken from the veins, they were all compared together. The urine had so deep a tinge, that it nearly resembled the pure tincture of rhubarb in appearance; the others had a tinge, although in very different degrees; the quantity of rhubarb they contained was estimated by adding tincture of rhubarb to alkaline water so as to produce corresponding tints. The infusion of spleen had a tint equal to sixty drops of tincture

tincture of rhubarb in two ounces of alkaline water: the serum of the splenic vein to fifteen drops: the serum from the left auricle of the heart, to three drops. The infusion of the liver gave no orange tinge, but had it not been obscured by the red particles of the blood, it must have been equal to that of the serum from the auricle.

Connecting membrane between the stomach & spleen examined.

The connecting membrane between the stomach and spleen was attentively examined, very few absorbent vessels were seen, and these were not in a turgid state, they were traced to the chain of glands situate near the edge of the spleen, which receive the absorbents of the stomach, but none were detected passing beyond the glands, nor did the glands admit quicksilver to pass through them towards the spleen.

Exp 2. Results similar, but in a less degree; perhaps from inflammation of viscera.

Exp. 2. The former experiment was repeated upon another ass, with similar results, but less strongly marked; the cause of this difference was explained by the abdominal viscera being in an inflamed state.

The urine was less impregnated with rhubarb, the infusion of the spleen had a lighter tinge, and the serum of the splenic vein had it in a still less degree; but evidently exceeding that of the serum from the vena cava inferior opened just below the diaphragm, which was substituted for the left auricle of the heart, with a view to vary the experiment.

Exp. 3. Similar.

Exp. 3. The same experiment was made on a third ass with similar results.

Exp. 4. Powdered rhubarb given.

Exp. 4. An ass that had been kept four days without water, and two without solid food, on the evening of the 8th of January, 1808, had a ball given it, containing half an ounce of powdered rhubarb; on the 9th, at seven o'clock in the morning, this was repeated; a third was given at nine o'clock, and a ourth at twelve. At two o'clock the ass was pithed, and four ounces of blood were taken from the splenic vein, and the same quantity from the left auricle of the heart.

Spleen much contracted.

The spleen was found contracted to half the size of those in the former experiments; when cut into the cells were small, and it required a magnifying glass to see them distinctly. The substance was compact, and bore a near resemblance

semblance to a portion of liver; so that in this state the blood vessels, particularly the veins, must have been much contracted in their diameters.

Other viscera.

The stomach contained about two ounces and a half of a gelatinous substance mixed with rhubarb, the small intestines were nearly empty, but the cæcum and colon contained several quarts of water, in which the rhubarb was more evident both to the sight and smell, than in the stomach.

The absorbent glands upon the edge of the colon were ranged in two rows, one on each side of the great vein, and were exceedingly numerous. In the space between these rows of glands, in some places twenty trunks of absorbent vessels could be readily counted, of a very large size.

Rhubarb in the urine.

The urine was impregnated with rhubarb, so as to acquire an orange tinge from the addition of the test; but the infusion of the spleen, and the serum of the different portions of blood, did not contain it in sufficient quantity to have the colour heightened by alkali.

Exp. repeated with similar results.

Exp. 5. The last experiment was repeated upon another ass. Two ounces of blood were taken from the splenic vein, two from the large vein of the colon, and two from the inferior vena cava in the lower part of the loins.

The spleen had the same appearance as in the last experiment.

The stomach contained nearly a pint of moderately solid contents, in which the rhubarb was very evident. The small intestines were nearly empty; but the cæcum and beginning of the colon contained several quarts of liquid, strongly impregnated with rhubarb.

The absorbent glands and vessels had the same appearance as in the former experiment.

The urine when tested was found impregnated with rhubarb.

The proportions of serum of the blood taken from these different veins, when tested by the alkali, appeared to be very much alike; at least that from the splenic vein was not more tinged than the others.

Spirituous liquors produce inflammation

Exp. 6. Having been informed by Mr. Sewell, that spirituous liquors, given in large quantities to horses, produce inflammation

inflammation of the brain, and sometimes death; and this information having been in some measure confirmed by an ass in a weakly state, that had taken half a pint of the spirituous tincture of rhubarb in the evening, dying in the night; I thought it right to make a comparative experiment with the infusion of rhubarb, to determine whether the result would be the same as with the tincture. of the brain in horses.

February 9, 1808. An ass had a pint of infusion of rhubarb given to it in the evening; the same dose was repeated at six o'clock in the morning of the 10th; and again at nine o'clock, and at twelve. At two o'clock the animal was pithed, and two ounces of blood were taken from the splenic vein, two from the vein of the colon, and two from the inferior vena cava in the lower part of the loins. Exp. 6. aqueous infusion of rhubarb given.

The spleen was found turgid, and large; when the cut surface was rubbed on white paper, the orange tint was very evident without any test applied to it, particularly so, when compared with a similar stain made by a section of the liver, in which there was no such tinge. Rhubarb found in the spleen,

In the stomach and duodenum, the rhubarb was found in large quantities; but none was met with in the cæcum. stomach and duodenum,

The urine was impregnated with rhubarb, the orange tint upon the application of the alkali being very distinct. urine,

At the end of twenty hours, the serum of the splenic vein had a tinge equal to four drops of the tincture of rhubarb in two ounces of alkaline water; that of the vein of the colon and vena cava was less distinct. and serum.

The effects of the infusion of rhubarb on the spleen, the serum of the blood, and the urine, corresponded exactly with that of the tincture in the former experiments, but was in a less degree of intensity. Effects slighter than with the tincture.

In the course of these experiments, an attempt was made to ascertain whether the blood in the splenic vein has a greater proportion of serum than in the other veins of the body, and the general results were in favour of such an opinion; but it will appear, from what follows, that the quantity of serum separated in twenty-four hours is by no means a just criterion of the proportion, which the blood contains. Quantity of serum apparently greater in blood from splenic vein: but what separates in 24 hours no just criterion.

Experiment 1. Three ounces of blood from the arm of a healthy Exp. 1.

healthy person were received into a graduated glass vessel, previously cooled to the temperature of 32°, three more into a second glass of the temperature of 50°, and three into a third at 70°. The three glasses were brought into a room, the temperature of which varied from 40° to 50°. At the end of nineteen hours, the serum was found in the following quantities.

In the glass at	32°	9 drams.
	50°	11
	70°	10

The blood did not flow so freely into the glass at the highest temperature, as into the other two.

Exp. 2.

Exp. 2. This experiment was repeated, and the serum examined at the end of forty-three hours.

In the glass at	32°	12 drams.
	50°	12
	70°	13

Exp. 3.

Exp. 3. It was repeated, and the serum examined at the end of 67 hours.

In the glass at	32°	11 drams.
	50°	11½
	70°	11½

Exp. 4.

Exp. 4. It was repeated, and the serum measured at the end of ninety hours.

In the glass at	32°	11½ drams.
	50°	13
	70°	10½

The blood did not flow so readily into the glass at the highest temperature as into the other two.

Most serum separates from blood received into a warm vessel, and flowing freely.

From these experiments it appears, that the serum separates in larger quantity, when the blood is received into a vessel at the temperature of 70 degrees, than at 50° or 32°: this, however, is prevented from taking place by the blood not flowing readily from the vein.

From the experiments on the spleen contained in this and the foregoing paper, the following facts appear to have been ascertained.

Spleen in two different states:

That the spleen is met with in two very different states, one

one which may be termed the distended, and the other the contracted; and that in the one its size is double what it is in the other. In the distended state there is a distinct appearance of cells containing a limpid fluid, distinguishable by the naked eye; in the contracted, these only become distinct when seen through a magnifying glass. The distended state takes place when the stomach has received unusual quantities of liquids before the animal's death; and the contracted state, when the animal has been kept several days without any drink before the spleen is examined. dependent on drinking.

That the trunk of the splenic vein (of the hog) is more than five times the size of the trunk of the splenic artery. Splenic vein 5 times as large as the artery.

That, when the pylorus is secured, coloured liquids pass from the cardiac portion of the stomach into the circulation of the blood, and go off by the urine; and while this is going on, the spleen is in its most distended state, and the colouring matter is found in its juices, although it is not to be detected in those of the liver. The colouring matter cannot therefore be conveyed to the spleen through the common absorbents of the stomach, which lead to the thoracic duct. Liquids pass from the cardiac portion of the stomach to the spleen.

That, when the pylorus is open, the colouring matter under the circumstances above mentioned is equally detected in the spleen.

That, when the spleen is in this state, the blood in the splenic vein has its serum more strongly impregnated with the colouring matter, than that of the blood in the other veins of the body; and when the stomach is kept without liquids, although colouring matter is carried into the system from the intestinal canal by the ordinary channels, no particular evidence of it is met with in the spleen or its veins. Colouring matter found in the serum from the splenic vein, if the stomach contain a liquid.

That the cæcum and the portion of the colon immediately beyond it are found (in the ass) to be at all times filled with liquids, even when none has been received into the stomach for several days, and there is a greater number of absorbent vessels for carrying liquids from the colon into the thoracic duct, than from any other part of the body. Blood vessels occasionally supplied with liquids from the colon.

The

The colon is therefore a reservoir, from which the blood vessels are occasionally supplied with liquids.

Mr. Sewell informs me, that the same observation applies in a still greater degree to the horse.

Liquids drunk reach the bladder in a few minutes.

That coloured liquids taken into the human stomach, under some circumstances, begin to pass off by urine in seventeen minutes, continue to do so for some hours, and then disappear; they are again met with in the urine, after the colouring matter is known to have arrived at the great intestines, by its passing off by the bowels.

From the above facts, the following conclusions may be drawn.

Liquids conveyed in part through the spleen.

That the liquids received into the stomach, beyond what are employed for digestion, are not wholly carried out of it by the common absorbents of the stomach, or the canal of the intestines, but are partly conveyed through the medium of the spleen into the circulation of the liver.

The communicating vessels not yet discovered.

The vessels which communicate between the stomach and the spleen have not been discovered; but if it is proved, that the colouring matter of the contents of the stomach is met with in greater quantity in the spleen, and in the vein which goes from that organ to the liver, than in the other veins of the body, there appears to be no other mode in which it can arrive there, but by means of such vessels; and the two different states of the spleen, which correspond with the quantities of liquids that pass from the stomach, are strongly in favour of the existence of such a channel.

Hence people in the habit of drinking have the spleen and liver diseased.

This communication between the cardiac portion of the stomach and the spleen will explain the circumstance of those, who are in the habit of drinking spirituous liquors, having the spleen and liver so frequently diseased, and the diseases of both organs being of the same kind.

The spleen not essential to life but its diseases injure digestion.

This organ is not essential to life, its office being of a secondary kind; but when it is materially diseased, or entirely removed, digestion must be disturbed. The extent to which this takes place cannot be accurately known from experiments on quadrupeds, and the instances in which the human spleen has been removed have not been attended to with sufficient accuracy, to afford an explanation of the effects that were produced on the stomach.

V.

Mr. Gilpin's Machine for raising Coals, Ore, &c.

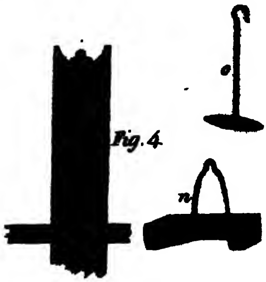


Fig. 4.

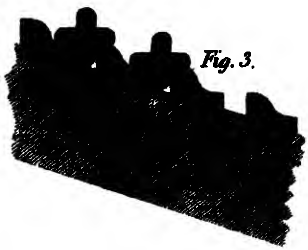


Fig. 3.

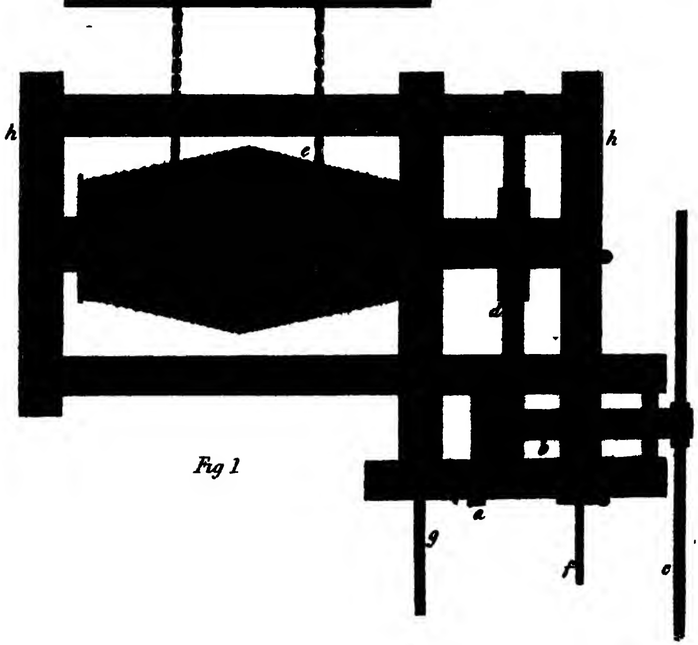


Fig 1

V.

Account of an improved Machine for raising Coals, or other Articles, from Mines: by Mr. GILBERT GILPIN, *of Old Park Iron Works, near Shifnal**.

SIR,

THE improvement of the machines in use for raising coal and ore from the mines has long been a desideratum of the Society for the Encouragement of Arts, Manufactures, and Commerce, and they have repeatedly offered a premium for this purpose.

Experiments on machines for raising coal required

Those in general use (from the increased expense of horse labour), are worked by a steam engine, attached to a crank of twenty-one inches radius, wedged on a shaft along with a fly wheel, eleven or twelve feet in diameter, and pinion wheel, of eleven teeth, which latter works in another of sixty-four teeth, on the shaft of which is a plain cylindrical barrel, from four to six feet diameter, and nine or ten feet long. Some have barrels formed of frustums of cones, (the perimeters of which are in the proportion of about five to four), united at their bases, and of various diameters. The axes of both kinds are placed at right angles with the centre line of the pit, and at each end a rope of six inches in circumference is made fast by a staple, which ropes work (in contrary directions at the same time) over two pulleys, placed in a frame parallel to each other, and at an equal distance from the centre of the pit; to the ends of these ropes the baskets of coal and ore to be raised are hooked.

Those in common use.

The simplicity of their general structure is such as, perhaps, not to admit of any considerable improvement; but the forms of the barrels are very defective.

Defect in the form of the barrel.

On putting one of these machines in motion each rope forms a triangle, the lines thereof from the pulley to the first and last coil, and the surface of the barrel, forming its

* Trans. of the Society of Arts, vol. XXV, p. 74. Twenty guin as were voted to Mr. Gilpin as a premium by the Society.

three

Cylindrical barrel.

three sides. Upon the cylindrical barrel the load always tends, from gravitation, towards the nearest point of contact with the centre of motion of the barrel, and, in consequence, the ascending rope at first bends around it in receding coils from the subtending side of the rectangle, diminishing their distances as they approach the nearest point of contact, (where the rope crosses the centres of the pulley and barrel at right angles), thereby leaving a great part of the latter uncovered by the rope, and hence the necessity of such long ones; afterward coiling hard against itself as it approaches the other side of the triangle, to its great injury in wear.

Conical in improper proportions.

The barrels formed of frustums of cones, united at their bases, the perimeters of which are in the proportion of about five to four, are equally defective, on account of the rope, for the reason before mentioned, binding hard against itself, and even sometimes, (in wet weather, when its rigidity is increased by absorption of water,) folding at first in receding coils, and afterward so hard against itself as to force those receding coils to slip suddenly towards the small perimeter of the cone, thereby making a large portion of the rope to descend the pit in an instant, breaking the rope by the sudden jerk, and frequently causing the immediate destruction of the men who may be ascending the pit at the time, or dashing to pieces the basket and its contents.

Danger.

Disadvantages.

Beside the unnecessary expense arising from the use of hempen ropes, and the breakage of chains when applied in the common way, the forms of the barrels are quite erroneous in principle. Some are cylindrical; others formed of frustums of cones united at their bases, without any determinate proportion in their perimeters, or regard to the weight of the rope or chain working thereon, both of which are absolutely necessary to acquire a maximum effect.

Proper proportions.

The convex surface of a frustum of a cone is equal to the convex surface of a cylinder of the same altitude, having its circumference equal to half the sum of the perimeters of the frustum: and circumferences of circles being to one another as their diameters, the surface of a barrel formed of two frustums of right cones (united at their bases), each 64 inches diameter at one end, 32 at the other,

and

and 54 long, which is the size we have adopted here, is equal to the surface of a plain cylindrical one, 48 inches diameter, and 108 long. Each will therefore bend the same length of cordage in a equal number of revolutions, and so far they are equal to each other; but they vary very considerably in the momenta required to work them.

Let $a =$ the weight of the basket of coal, and $b =$ that of the descending part of the chain; then, on the cylindrical barrel, when the former is hooked to the end of the latter, and eased from the bottom of the pit (the opposite chain being bent on the barrel), $a + b =$ the counterpoise required at 24 inches radius; and when it is wound up to the top (the descending part of the opposite chain hanging down the pit), $a - b =$ the counterpoise required at the same radius. Force required with a cylinder:

On the barrel formed of frustums of right cones, when the load is eased from the bottom of the pit, it and the chain are suspended from one of the smaller perimeters (the opposite chain being bent on the barrel), $\frac{a}{2} + \frac{b}{2} =$ the counterpoise required at 32 inches radius; and when it is wound to the top of the pit, it is suspended from the larger perimeter of one frustum, while the descending part of the opposite chain is hanging down the pit from the smaller perimeter of the other, and in that position $a - \frac{b}{2} =$ the counterpoise required at the same radius. and with two frustums of cones.

Consequently, by supposing a, the weight of the basket of coal, to be 800lbs. and b, the weight of the descending part of the chain, 400lbs. (these are the weights which we have adopted here), we have the counterpoise required upon the cylindrical barrel, at 24 inches radius, 1200lbs. when the basket of coals is at the bottom of the pit, and 400lbs. when it is at the top; but upon the barrel formed of frustums of right cones, the counterpoise required at 32 inches radius is 600lbs. in each position. And as the counterpoise required is in inverse proportion to the length of the radius at which it is applied, we have 24 : 32 :: 600 : 800lbs. the counterpoise required upon the barrel formed of frustums This applied in practice.

 of

of right cones, at 24 inches radius. Again, as the descending part of a chain + a basket of coal of double its weight, unbending out of equidistant grooves from the base of a frustum of a right cone towards its smaller perimeter, balances in every revolution of the barrel a chain of equal weight + a basket of coal of double its weight, bending into equidistant grooves from the smaller perimeter of a similar frustum towards its base, the counterpoise required must be equal in all parts of the descent.

Maximum of effect.

So that by making the weight of the basket of coal to that of the chain, and the perimeters of the frustums of cones, which form the barrel, to each other in the proportion of two to one, *a maximum is obtained, by which a barrel of this description requires one third less momentum, (and consequently one third less expense), to work it than a cylindrical one.*

Barrels.

The barrels are made by nailing two or three inch planks upon wooden or iron curves, as in the common way; and afterward folded spirally with wrought iron tire, so as to leave a vacancy of about half an inch between each fold, for the lower part of the ellipses of those links of the chain which work vertically to move in, and keep the coils at an equal distance from each other.

Iron work.

The wrought iron tire is of two kinds, the one for conical, and the other for cylindrical barrels; the cross section of that for the barrel formed of frustums of cones is nearly a parallelogram $1\frac{1}{4}$ inch by $\frac{5}{8}$ths; out of the upper part of which about one fourth of an ellipsis is taken, to form a horizontal bearing for those links of the chain, which lie flat upon the tire; the cross section of the latter is a rectangle, $1\frac{1}{4}$ inch by $2\frac{1}{2}$ inch. Both are rolled into their proper form, and holes of a quarter of an inch diameter punched therein, at a foot from each other, for the purpose of nailing them to the planking of the barrels.

Chains working in grooves a recent practice.

As the method of working chains in grooves has only been in use about three years and a half, it is impossible to give a certain idea in respect to their durability. In all that time not a single link has broke, or the least accident occurred therefrom, though Messrs. T. W. and B. Botfield, have nearly three thousand feet in daily motion at this manufactory.

tory. The wear has also been so trifling, that I conceive they will sooner fail from oxidation than attrition: for although the machines for raising coal and ore from the mines are in use twelve hours in the day, the brown oxide of iron formed upon the links by exposure to the atmosphere is seldom disturbed by the motion of the chain.

The method of folding wooden barrels with wrought iron tire does away the necessity of cast iron ones, and may be applied to every wooden barrel now in use at a small expense, as may be seen by the estimate which is subjoined. Cast iron barrels unnecessary.

There are now at work in the mines of this manufactory four machines, with wooden barrels folded with wrought iron tire, one cylindrical, and three formed of frustums of cones, raising upwards of eight hundred tons of coal and iron ore per week from pits of about eighty yards deep; and three others are in hand.

I look forward with confidence to the general substitution of chains for hempen ropes at all our mines and manufactories, a matter of importance to the British empire, as it will considerably lessen the consumption of hemp, and render it more abundant for the exigencies of the Navy. Chains used instead of hempen ropes.

Wishing to give this method of working chains all the publicity in my power, I will obviate all apparent (for there are no real) difficulties, which may occur to any person in their application, on his stating them in a letter *post paid* addressed to me here.

I am, Sir,

Your most obedient Servant,

GILBERT GILPIN.

Expense of tarred ropes for a machine for raising coal and ore from a pit eighty yards deep, for three years and four months. Comparative estimate of expense.

	£	*s.*	*d.*
Ten ropes each 110 yards long, 6 inches in circumference, and 5lbs. per yard, 5500lbs. at 8*d.* per lb (Ropes.)	183	6	8
Deduct 10 worn out ropes 2750lbs. at 1*d.* lb	11	9	2
Net expense of ropes for 3 years and 4 months	£171	17	6

 Expense

Expense of chains for a machine for raising coal and ore from a pit eighty yards deep.

Chains.

Two chains each 110 yards long, formed of $\frac{3}{8}$ inch iron, 28 links to the yard, and weighing 5lbs. per yard, 1100lbs. at 6*d*. per lb........	27	10	0
180 yards of wrought iron tire, with the holes punched therein weighing 7lbs. per yard, at 1*s*. 6*d*. per yard........................	13	10	0
540 nails for the tire, 27lbs. at 6*d*. per lb......	0	13	6
Workmanship, nailing the tire on the barrel, 180 yards at 2½*d*. per yard.....................	1	17	6
	£43	11	0

The above chains and tire have been at work three years and four months, and do not appear to be one fourth worn.

Ropes retained in part on account of the men's prejudices.

Messrs. T. W. and B. Botfield annex a certificate, that they have now at work at their mines four of Mr. Gilpin's machines, one with a cylindrical barrel, and three formed of frustums of cones; which machines they conceive to be superior to any hitherto known or in use, and producing their effect at a much less expence. To this Mr. Gilpin subjoins

You will please to observe, that of the four machines now in use, two only work with *two chains each*, and they are both formed of frustums of cones; the other two, the one with a cylindrical barrel, and the other a frustum of a cone, have each a chain at one end, and a patent flat rope at the other. We are induced to adopt the latter plan, to do away by *degrees* the prejudices, which miners and colliers have imbibed against chains, from accidents which they have been witnesses to in the common way of working. Though the causes of similar accidents are entirely done away by the new method of working, some little of the old prejudice remains; a thing not to be wondered at, when we consider the uninformed state of this description of men, arising from a life spent in the dark recesses of mines; and, as it were, cut off from the rest of society.

From

From the uniformity and safety of the new method, their prejudices against chains are, however, rapidly wearing away, and I have no doubt, that in a few years they will even be preferred. It is certainly more reasonable to suppose, that this will be the case, from the superiority which iron holds in point of strength of materials, than that ropes even should have been known, (at least in the mines,) had the new method of working chains been in use prior to the introduction of hemp. These wearing out.

Reference to the Engraving of Mr. Gilbert Gilpin's improved Machine for raising Coal, Ore, &c. Pl. 3, fig. 1, 2, 3, 4.

Fig. 1. *a.* A crank to which the connecting rod is fixed to attach the machine to the steam-engine which works it. Explanation of the plate.

b. A wheel of 13 teeth, wedged about the same shaft with the crank, and which works into the wheel *d.*

c. A fly wheel 11 feet in diameter, wedged upon the same shaft as the wheel *b.*

d. A wheel of 64 teeth, wedged upon the same shaft as the barrel, into which the wheel *b* works.

e. A wooden barrel, formed of two frustums of cones united base to base, and folded spirally with wrought iron tire, which keeps the links of the chains at right angles with each other, and with the grooves in the pulleys.

ff. The reeling-post and its lever, for disengaging the barrel from the steam-engine, when the men are to be let down into the pit by means of the break.

gg. A break wheel, break and lever, for regulating the velocity of the barrel when disengaged from the steam engine, and in the act of lowering the miners into the pit.

hh, The frame on which the machine is erected.

ii. Fig. 2. The pit-frame, for supporting the pulleys.

k. The pit represented by a circle, part of which is shown open, and part by dotted lines.

ll. Two grooved pulleys, over which the chains, extending a considerable length from the barrel *a* work in parallel lines.

m. The carriage (called a tacking in Shropshire) on which the

the coal and ore are landed from the chain at the pit head, moving on four small iron wheels.

nn. Baskets on which the coal and ore are raised from the pits.

o. The hook which goes into the staple of the basket to draw it forward when lowering on to the tacking.

After the basket is lowered, the tacking is drawn forward by two girls at the edge of the frame, which is laid level with the ground on its outside, and near to which the coal and ore are loaded into waggons, and afterward drawn upon iron rail-ways to the furnaces, forges, &c.

Fig. 3. A section of a part of the barrel and tire, showing the manner the links of the chain lie on it, on a scale of 3 inches to the foot.

Fig. 4. A section of the pulley, with a link of the chain lying in it.

In a large machine the barrel is fixed 24 or 25 yards from the pit.

VI.

Remarks on Mr. GOUGH'S *Essay on Polygonal Numbers: By* P. BARLOW, *Esq.*

To the EDITOR of the PHILOSOPHICAL JOURNAL.

SIR,

On Fermat's proposition on polygonal numbers.

IN your number for July, the first article is an essay by John Gough, Esq., in which he has attempted the demonstration of a very curious and general property of numbers; but as it appears to want that perspicuity and simplicity, which are the distinguishing beauty of mathematical reasoning, I have drawn together the following observations upon it; which, if you think proper to insert them, will give your readers an opportunity of judging of its merits, more particularly than they may have hitherto done. It will also

a so afford Mr. G. an opportunity of explaining the ambiguous parts; and will at the same time much oblige

On Fermat's proposition on polygonal numbers.

Yours, &c.

Royal Mil. Academy,
Sept. 13, 1808.

P. BARLOW.

The curious theorem, which Mr. Gough has undertaken to demonstrate, was first announced by Fermat, in one of his notes at page 180 of his edition of Diophantus; and the demonstration for the particular case of squares was given first by Lagrange, in the *(Mém. de Berlin,* 1770*)*, and afterwards in a simpler form by Euler, in the *(Acta Petrop. Ann.* 1777*)*, as we are informed by le Gendre, in his *Essai sur la Théorie des Nombres*, at page 202; where there is also given a demonstration for the same particular case. Le Gendre has likewise in another part extended it to triangular numbers, and this is the most that has ever been done by any mathematician. If therefore the ingenious author of the abovementioned essay has failed in his demonstration, he has the satisfaction of having failed in an attempt, in which many of the ablest mathematicians in Europe have succeeded no better than himself; and if, on the contrary, he can clear up those parts, which appear at present to be defective, the greater degree of merit will be due to his ingenuity and ability, of which I have always entertained the highest opinion: and I feel confident, that he will not mistake my intentions in the following criticism, but rather attribute it to my love for mathematical truth, than to any invidious desire of criticising his paper.

The first three propositions and their corollaries are in themselves correct, although I am at a loss to see in what manner they are intended to be applied to the general demonstration. The first part that I shall examine is the conclusion drawn at cor. 2, prop. 4. In cor. 1 of the same prop. it is proved, that e, which is taken to represent any aggregate of polygonals of the denomination m, is of the form $e = p + \overline{m - 2}.s$; and then in cor. 2, having shown that every natural number is also of the same form, $p + \overline{m - 2}.s$, the converse of the prop. is inferred to be true likewise;

On Fermat's proposition on polygonal numbers.

likewise; namely, that every number is the aggregate of polygonals. Now it is easily seen, that this is false reasoning. Our author might, with as much propriety, have said, that every natural number is either even or odd, and every aggregate of polygonals being also either even or odd, therefore every natural number is the aggregate of polygonals: or, to put it in a stronger light, every natural number is of the form $p + \overline{m - 2}.\, s$; and, every square number being also of the form $p + \overline{m - 2}.\, s$, therefore every natural number is a square number. No person can for a moment fail of detecting in those two last cases the fallacy of this reasoning, nor of perceiving the strict analogy it bears with that made use of in the cor. abovementioned. It is to be observed, that I do not object to the conclusion, but to the manner of obtaining it; for all that is drawn from the first four propositions and their corollaries might have been granted at first as a postulate, if any use could have been made of it in the general demonstration.

For unity is a polygon of every denomination, and every natural number is composed of a number of units, therefore every natural number is composed of a number of polygons of any denomination m, consequently every natural number is either a polygon of a given denomination m, or may be resolved into polygons of that denomination; the number of those polygons being unlimited, as in the corollary alluded to.

The next place, where any conclusion is drawn, is in the cor. to prop. 6, where it is said, that *If* $e = y + t$, can be resolved into polygons, the number of which $= m - f$, $e + f$ may be resolved into m polygons of the same denomination. Now either this supposition is necessary to complete the demonstration, or it is not: if it is not necessary, it ought to have been omitted; if it is necessary, it ought to have been shown (but it no where is in the demonstration) that $e = y + t$ may be resolved into $m - f$ polygons, because the conclusion depends upon this supposition, and if the supposition is *true*, the conclusion is *true*: on the contrary, if the supposition is *false*, the conclusion must necessarily *be so likewise*. This language is at all events too vague for mathematical reasoning. I am willing to

to allow that, *if* e *can* be resolved into $m-f$ polygons, $e+f$ may be resolved into m polygons; but *if* e *cannot* be resolved into $m-f$ polygons, what proof have we, that $e+f$ can be resolved into m polygons? And that there are many such cases is evident: thus, 14 cannot be resolved into less than *three* or m triangular numbers, nor 23 into less than *four* or m squares.

On Fermat's proposition on polygonal numbers.

This ought to be explained, the importance of the proposition demands it, the last labours of *Euler*, *Lagrange*, and *le Gendre* demand it also, and a few more pages may very well be afforded to complete the demonstration.

The remaining part of the essay goes on to show, how any given number may be resolved into polygonal numbers of any given denomination; but, from the examples there given, it does not appear to possess any advantage over the usual method of trials; and even if it did, it is of no use in demonstrating the proposition, for showing how a thing is to be done is very different from showing it may always be done.

Upon the whole therefore we may conclude, that for the *present*, the celebrated theorem of Fermat is without a demonstration, and that its importance, as containing one of the most curious properties of numbers, renders it worthy the attention of mathematicians.

VII.

Some farther Remarks on the Doctrines of Chance, in a Letter from a Correspondent.

SIR,

HAVING observed a letter in the last number of your valuable publication, from a correspondent who has assumed the signature of Opsimath, in which some doubts are expressed respecting the elementary Doctrines of Chance, and a request to yourself, or any of your correspondents, either to confute or to confirm his objections, I have ventured to offer the following remarks; though certainly with some diffidence, being

Certain doctrines of chance questioned by a former correspondent.

being apprehensive from your having declined favouring us with your remarks, that your view of the subject might not be very dissimilar to your correspondent's.

The case objected to.

Opsimath admits de Moivre's first case, viz. that if any one were to undertake to throw an ace in one throw with one die, he would have $\frac{1}{6}$ of all the possible chances in his favour, and the remaining $\frac{5}{6}$ against him: but objects to the second case, viz. that, if he were to undertake to do it in two throws with one die (or, which is certainly the same thing, in one throw with 2 dice) that the chances in his favour are $\frac{11}{36}$ and $\frac{25}{36}$ against it; alleging as a reason, that two equal chances are twice as good as one, and that of course it should be $\frac{12}{36}$ instead of $\frac{11}{36}$.

Attempt to defend it.

This reasoning is correct if the chances are of equal value: but this I apprehend is not the case, the second chance being less than the first by the probability of the first's succeeding; and as a confirmation of de Moivre's doctrine being correct, it appears by the following statement, that of all the 36 possible combinations with 2 dice, there are but 11 throws which give an ace or any other particular number. Let 1 die be called A, the other B; then may be thrown with

A	B	A	B	A	B	A	B	A	B	A	B
1	1	2	1	3	1	4	1	5	1	6	1
1	2	2	2	3	2	4	2	5	2	6	2
1	3	2	3	3	3	4	3	5	3	6	3
1	4	2	4	3	4	4	4	5	4	6	4
1	5	2	5	3	5	4	5	5	5	6	5
1	6	2	6	3	6	4	6	5	6	6	6

and again, as the chance of throwing an ace with one die is admitted by your correspondent to be $\frac{1}{6}$, and of not doing it $\frac{5}{6}$, the chance of not doing it with either of two dice is $\frac{5}{6} \times \frac{5}{6} = \frac{25}{36}$, and this subtracted from unity, which represents the certainty of an ace being either thrown or not thrown, gives $\frac{11}{36}$ as above.

The argument pursued.

Opsimath farther objects to this statement, and says, if we proceed according to the above method, the probability of throwing an ace with one die in 6 throws does not amount to $\frac{6}{6}$, or a certainty. Nor should it: for were this the case, he might undertake to pay any sum, provided he did not do

do it in 6 throws with 1 die, or in 1 throw with 6 dice, which I think he would be very unwilling to do. The fact is, that out of the 46656 possible combinations with 6 dice, there are only 31031 throws that produce an ace, or any other particular number; which, if he will take the trouble, he may convince himself of, by trying all the combinations, as in the preceding statement of the 2 dice, or according to the method before given, viz.

Probability of not throwing an ace

with 1 die	$\frac{5}{6}$	of doing it	$\frac{1}{6}$
with 2 dice $\frac{5}{6}\times\frac{5}{6}=$	$\frac{25}{36}$	....	$\frac{11}{36}$
with 3 dice $\frac{5}{6}\times\frac{5}{6}\times\frac{5}{6}=$	$\frac{125}{216}$	....	$\frac{91}{216}$
with 4 dice $\frac{5}{6}\times\frac{5}{6}\times\frac{5}{6}\times\frac{5}{6}=$	$\frac{625}{1296}$	....	$\frac{671}{1296}$
with 5 dice $\frac{5}{6}\times\frac{5}{6}\times\frac{5}{6}\times\frac{5}{6}\times\frac{5}{6}=$	$\frac{3125}{7776}$	....	$\frac{4651}{7776}$
with 6 dice $\frac{5}{6}\times\frac{5}{6}\times\frac{5}{6}\times\frac{5}{6}\times\frac{5}{6}\times\frac{5}{6}=$	$\frac{15625}{46656}$	....	$\frac{31031}{46656}$

Similar reasoning applied to two halfpence.

With respect to throwing a head with a halfpenny in 2 throws (or with 2 halfpence in 1 throw, being the same thing) it ought, according to his view of the subject, if I understand him right, to amount to a certainty; as there are but two ways in which a halfpenny can be thrown, and there being two halfpence to do it with. He appears however to be satisfied with de Moivre's value of the chance, viz. $\frac{3}{4}$, which is the true one, for in the 4 ways in which 2 halfpence may be thrown, there are only 3 which give a head; for with the first

may be thrown a head, and with the second a head,
head, ... tail,
tail, ... head,
tail, ... tail.

I am, Sir,

Your constant reader,

and most obedient servant,

B. H.

10, Millman Street, Bedford Row,
14 *Sept.* 1808.

REMARK.

REMARK.

The mistake has arisen from confounding several throws of one die with one throw of several dice.

B. H. appears to be led into an errour by supposing, that the chances for throwing an ace in six throws of one die and six throws of another, are the same thing with the chance of throwing an ace in six throws of two dice; but this is not the fact. Six throws of one die and six throws of another are clearly equal to twelve throws of one die, and this chance I apprehend will not be denied to be equal to two. In throwing two dice thirty-six times, it appears by the table of B. H. himself, which is perfectly accurate, that the thrower may calculate upon throwing twelve aces, as he might by throwing one die seventy-two times: but here is the difference; in throwing one die seventy-two times, he has a right to reckon on an ace being turned up in twelve of the throws; in throwing the two dice thirty-six times however, he can reckon on no more than eleven throws in which an ace will be turned up, because in one of the throws two aces will come together, and consequently one will be lost, which evidently cannot be the case when the two dice are thrown in succession.

With regard to the throws of a halfpenny the reasoning is precisely the same; nor does Opsimath appear more inclined to acquiesce in the assertion, that the chance of throwing a head with one halfpenny in two throws is only $\frac{3}{4}$: though he would probably allow this to be the true value of the chance of throwing a head at one throw with two halfpence. C.

VIII.

Description of a secure Sailing Boat, or Life Boat. By Mr. CHRISTOPHER WILSON, *Richard Street, Commercial Road*.*

SIR,

Neutral built self-balanced boat.

HEREWITH you will receive drawings of a neutral built self-balanced boat, with an explanation, which I request

* Trans. of Soc. of Arts, vol. XXV, p. 55. The gold medal was voted to Mr. Wilson for this invention.

quest you will have the goodness to lay before the Society for the Encouragement of Arts, &c., for their inspection and approbation. I have made the explanation as clear as I can. Its construction will obviate the danger of its being overset by persons crowding on one side, in getting in or out of the boat; it will facilitate the landing of men on shore or in boarding ships, and will carry a much greater press of sail without danger.

Its advantages.

As to the building part, I think that may be easily understood. My boat was made by men that had never before seen a boat built, and I flatter myself the Society will approve of it.

I am, Sir,

Your most obedient humble servant,

CHRISTOPHER WILSON.

An Explanation of the Engravings of the neutral-built self-balanced Boat.

By the term neutral is meant, what is neither of the two present modes now in use, i. e. clincher and carvel, but both united, viz. clincher in the inside and carvel on the outside, which neutralizes both the two into a third; and as every thing has a distinguishing name, I have taken the liberty to present it to the public, under the name of a neutral boat.

Method of its construction.

The two modes of clincher and carvel-built have each their separate advantages and disadvantages in regard to each other.

I shall begin with the clincher first. As the sides of the planks are firmly fastened to each other, by lapping over and rivetting, they are much stronger than if the edges only butted; and they have the property of being made tight without caulking, only in the huddings and keel seams, and are much lighter than carvel-built boats, and more adapted for many uses; besides saving the difference between thick and thin plank. But they have their disadvantages also; in the first place, both unfair sides and unfair water lines, which makes them liable to be injured by other bodies they come

Advantages of clincher building.

Its disadvantages.

come in contact with, and have the edges of the planks broke so as to make a leak*, which would not happen to a smooth-sided boat, neither can the uneven side move so well through the water, on account of its various resistances†. They have also this disadvantage, that if damaged, they require the skill of a professional workman to repair them.

Advantages of carvel building.

The carvel built boats have the advantage of having smooth sides and fair water lines, together with having the planks of an equal thickness all over the boat, which makes them less liable to receive injuries when meeting with other bodies, and more adapted to move in the water, by their fair sides and fair water lines. They are also more readily repaired: if a professional boat-builder is not at hand, it can be done by a common shipwright, or any workman that is used to wood work.

Its disadvantages.

But they have also their disadvantages; in the first instance they are under the necessity of being built of plank of a great thickness to stand caulking; at the same time they require larger timbers, which makes them heavy and unfit for many uses, and also a great consumption of timber on account of the thickness of the plank necessary. They are also more subject to leaks from various causes than clincher-built boats.

Neutral building.

We will now look to the neutral system, and see if both their advantages are not united, and both the disadvantages got clear of.

Pl. IV, fig. 2, shows the section of the fore part of a boat. The longitudinal slips are represented lighter coloured, and placed over the joints where the edges of the planks meet; they must be rivetted on to each adjoining plank, near the edge, in the same manner as clincher-built vessels, with a sufficient quantity of blair, made of tar and flocks, such as is in common use in the north of England, (or any

* In the next paragraph but one carvel built boats are said to be more subject to leaks. C.

† This does not appear to be the fact. Clincher built vessels are so superior to others in sailing, that, by an act of parliament passed many years ago for the prevention of smuggling, they were declared illegal beyond certain dimensions. C.

other

other caulking) between the slips and planks, which will always keep them tight, as long as the boat remains unstaved, or the planks not worn through. These slips, each being rivetted to the two adjoining edges of the planks, as shown in Fig. 4, will make the joint as strong as the joint of a common clincher-built boat, and as tight, without the risk of any external damage. These joints have also this advantage, that the planks will not have their sides bevelled off, but be of an equal thickness from edge to edge, which is not the case in clincher-built vessels, for at the ends they are half bevelled away, so as not to bear clenching. By the neutral system two inches in the breadth of each plank will be saved in the laps, which may be considerable in the conversion of plank. I set little value on the slips, as there is always a sufficiency of waste in cutting the planks to a proper form.

A boat of this construction has all the strength of one clincher-built, and can be made as light or lighter. It is free from the disadvantages of irregular outsides, and from the difficulty of repairing, which in this can be performed by any common workman in wood, as I have found by experience. A boat built this way has a fair and smooth outside, it has all the advantages of a carvel-built one, at the same time it is clear of the disadvantages of being loaded with unnecessary wood, which makes the carvel-work very heavy, the liability of leaks, and frequent want of caulking. There is one evil, which both carvel and clincher built boats have in common, that of having keel seams, and a vacancy between the sand or garboard streak, and the upper part of the keel, which soon gets filled with dirt, and remains so, which naturally retains moisture, and speedily rots the wood. In this mode that evil is removed, by having the midship plank bolted on to the keel, wide enough to come over each side of the keel to clinch the slips on, this not only removes the evil, but saves a great deal of trouble in making the rabbets in the keel, and various bevellings in the sand streaks, which must be done by a good workman.

Common defect above the keel remedied.

These boats require no larger timbers than common clincher built boats, as the timbers need no greater notches, but

but with this difference, that these timbers will catch the slips that are rivetted over the joints of the planks each way, and so the timbers and slips will brace one another, and add an additional strength; but in the clincher built boats, the timbers catch the laps of the seams only one way, and consequently form no brace whatever.

Applicable to boats, barges, &c.

All I need to explain farther on the neutral system is its application. It can be applied to all open boats, of whatever form or use, to all coal and other barges, lighters, or any vessels used in rivers or canals, and also to all large cutters and luggers, which are now clincher built.

Explanation of Pl. IV, fig. 1, 2, 3, 4.

Explanation of the plate.

Fig. 1, is a bird's eye view of the boat, showing the projecting balance bodies, or hollow sides *a b*, one of which, *a*, is left open to show the partitions, which are placed opposite to each timber, and are water tight; by this means, if one or more should be broken, the rest would keep the vessel buoyant. These partitions gradually lessen towards each end, where the planks unite, so as to make a similar appearance to any other boat when in the water.

Fig. 2, shows the depth and form of the cells or hollows, as they appear in a section of the boat; also the manner in which the slips are placed over the joinings, or seams of the planks.

Fig. 3, is a perspective view of the boat, in which *a b* show the projecting balance bodies, or hollow sides, which would render the boat buoyant if her bottom was staved in. *c*, the lower part or body of the boat, from which the projections commence; *d*, the keel.

Fig. 4, shows the manner in which the planks or timbers of the boat are united; *e f*, are two planks of the boat; *g*, the slip of wood placed over them, and secured to them by the rivets *h h*.

The section (Fig. 2), will best explain the nature and utility of the self-balanced boat. The balance bodies form two separate holds, to put any thing in, such as provision, arms, &c., which are wanted to be kept dry, having locker lids, to open at the top of the different partitions in the holds, as fancy or utility may require; or part of them

may

Mr. C. Wilson's secure Sailing Boat or Life Boat

Fig 1

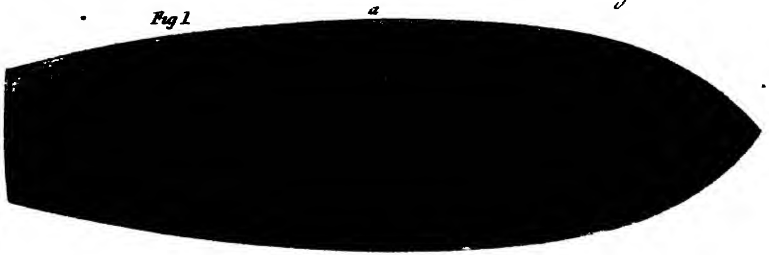


Fig 2

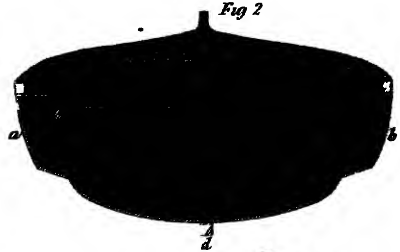


Fig 3

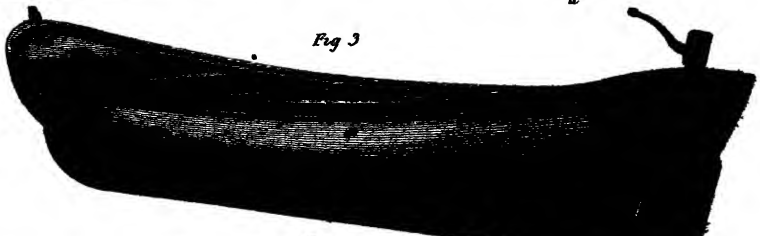
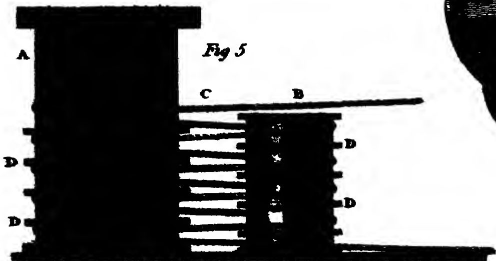


Fig 4

Fig 5



Mr. J. W. Boswell's improved Capstan

may be filled with cork shavings, and by that means, if the boat should happen to fill by any accident, she cannot sink.

In the boat I have altered for Government, the balance bodies (if the interior of the boat was filled with water) would exclude as much water, between the inside of the boat and the outside, as is equal to a body of water of 1 tun, 17 cwt, 2 qrs, which is a great deal more than the weight of men that will go in her, consequently they can run no risk whatever of being drowned; and even if she had a hole through her bottom, she would always keep a sufficient height out of the water either for rowing or sailing.

Boats altered for Government.

But the main object is to make her sail and row much faster than other boats, and both on calculation and trial my boat will be found to sail much faster, and with much less danger than other boats.

I now come to the advantage of rowing.—As the balance sides project a foot beyond the resisting part in the water, there is that leverage on the boat (over a common one), and also the same in the length of the loom of the oar, that is in the inside from the gunwale of the boat, which allows the whole of the oar to be lengthened, and by that means it describes a larger circle in the water, and makes a longer pull: the oars for the Government boat I have made are lengthened from 14 to 18 feet.

Advantage of rowing with a longer lever.

The experiment of having two spars fixed at a distance from a boat's gunwale, and the oars to work from them, has often been tried and found to answer, but this has a great advantage over that method.

This may be effected by projecting spars.

There is another advantage or property which this boat has, she cannot roll at sea, but always keeps a level position as far as the surface of the sea will allow; she may heel but not roll, as the balances are always ready to catch either way, and the opposite one assists the other by its weight out of water and gravitation; neither can this boat pitch like another, for the balance bodies being out of the water, and the breadth of six feet only in the water, it can only act with a gravity on the water, equal to a boat of the weight of six feet but as the resistance of the water upwards equal to a boat of eight feet wide.

Will not roll, and will pitch but little.

Or I may make this mechanical simile: Suppose a workman uses a chissel to smooth a surface of wood, by laying too great a stress on the tool it will go too far into the wood for him to force it along in the direction wanted, but put that chissel into a stock like a plane-stock, and set it to the depth required, then the stock will prevent its going too far in, and he can work easily though the plane be pressed on ever so hard. A view of the engraving will elucidate this comparison, as the balance bodies lie parallel with the surface of the water lengthways. The national importance of such boats I leave to the public to decide. I must here observe, that my plan contains two distinct and separate improvements, viz. my neutral mode of building, and the application of the balance bodies.

Two separate improvements of different application.

The first improvement relates to the *building* of boats, barges, &c., in general. The second is only partial, and applicable to boats of peculiar descriptions or uses; that is, all such as are wanted for dispatch, safety, or pleasure, or occasionally for life boats: as there can be no question of the self-balanced boats, built upon my plan, rowing and sailing faster than other boats, and they may be used to go to sea when others cannot; but the application of the balance bodies is not meant as a general one, as it is not fit for vessels of burden that are sometimes light, and at others heavy laden, when the difference of the draught of water is considerable.

CHRISTOPHER WILSON.

Opinions of the advantages of this mode.

CERTIFICATE.—We whose names are hereunto subscribed have examined the boat building on Mr. Wilson's plan, (which he calls the neutral plan) and are of opinion, that it will be attended with many advantages.

The boats can be built as light as those that are clincher built, preserving a smooth surface, and will not require caulking; and they can be easily repaired by any carpenter.

The advantage this boat possesses by having air gunwales are obvious, and from the partial trial we have had of the boat's sailing which he has altered, we are of opinion, that his

his improvement in the keel and formation of the boat's bottom will give her greater stability than other boats of the same dimensions, with the properties of sailing well and drawing very little water.

MALCOLM COWAN, R. N.
JAMES NICOLSON, R. N.

GENTLEMEN,

PERMIT me to present my thanks and acknowledgments for the truly polite and distinguished manner in which (though a stranger) you have permitted me to visit your Committee; the Society of which the same is formed I hold in the highest estimation, and have deeply to regret the distance, that prevents my offering myself a candidate for a seat among you.

The last time I had the honour of attending your Committee, Mr. Wilson's new life boat became the subject of discussion, the operation of which you did me the honour of requesting me to acquaint you of as soon as an opportunity presented itself for a fair trial of her at sea.

Trial of the boat in a gale of wind.

About three o'clock in the afternoon of Friday last, the tide being about quarter flood, and the wind at south-west, blowing excessively hard, an object was discovered in the offing at about two leagues distance, bearing from the piers of Newhaven W. S. W., which had the appearance of a vessel waterlogged, and with only her foremast standing. This induced Mr. Thomas Tasker (the person whom I appointed master of the boat, and which I have named the *Adeline)* with seven others, to put to sea, with a view of rendering assistance to the supposed distressed vessel, and although the breakers were tremendous, and the sea without them running very high, the boat under the management of the crew beforementioned, ranged as coxswain, six sitters, and a bowman, went out of the harbour in a very lively style, and soon came up with the object in pursuit, which proved to be a beacon, or lighthouse, of a singular construction, triangularly built, and clench-board covered in its floating case, with a mast rigged out in the centre of

one of the sides, and supposed to have broken adrift from the enemy's coast by the boisterous weather: finding its magnitude too vast for their strength to tow, and the evening approaching, they returned. Numbers of persons were assembled on the piers to witness the action, power, and performance of the boat, who were highly pleased and gratified. I was not present myself, but the next morning one of the crew was sent to me from Newhaven to this place, who stated, that the whole of them were so fully satisfied with the safety and superior powers of the boat, that they shall not be afraid to put to sea in any weather, when the distresses of their fellow creatures claim their exertions and assistance. They particularly observed, she, with the six oars manned, pulled extremely light and easy through the water, and that though the breakers they pulled through, and the heavy seas they rode over were awful, she did not ship ten gallons of water the whole trip, neither were the men wet on the seats. We have now at Newhaven one of Mr. Greathead's boats, provided by subscription, but from the difficulty of getting her to sea, and her weight and construction rendering it almost impossible to pull her through the broken water, it is very improbable she will ever be used.

Mr. Greathead's boats much inferior.

My opinion is, that Mr. Wilson's boat will answer. Its cost I conceive will exceed £150, including the building and fitting her out.

I have the honour to subscribe myself with the greatest respect,

Gentlemen,

Your obliged and most

obedient humble servant,

WILLIAM BALCOMBE LANGRIDGE.

P. S. I should have observed, that the crew pulled her *stern on* at every sea, and that such water, as in general fills over the bow of ordinary boats, is received by the fore part of her flammings, or floor of extended sides, and sent or dispersed sideways.

IX.

IX.

Description of a Capstan, that works without requiring the Messenger or Cable coiled round it to be ever surged. By J. WITLEY BOSWELL, *Esq., of Clifford's Inn**.

SIR,

I Request you will lay before the Society of Arts, &c. the model of a capstan contrived by me, which works without requiring the messenger or cable coiled round it to be ever surged, an operation necessary with common capstans, which is always attended with delay, and frequently with danger.

Capstan that does not require the messenger or cable to be surged.

Capstans of this kind can be made by a common shipwright, and would not be liable to be put out of order. They also would not occasion any additional friction or wear to the messenger or cable, in which particulars they would be superior to the other contrivance hitherto brought forward for the same purpose; they also would much facilitate the holding on.

The great loss of time and great trouble, which always attend applications to the Navy Board, prevent my attempting to bring the matter before the public through that channel, though I have had the most unequivocal approbation of the capstan from the two gentlemen of that board best qualified to judge of it. I mention this, least it might be thought, that my not applying there first was from any doubt of the goodness of the invention. If the Society should approve of the capstan, I will draw up a more minute account of it for publication.

Reasons for not applying to the Navy Board.

I am, Sir,

Your very humble servant,

J. W. BOSWELL.

SIR,

I Have examined your model of a capstan, which is calculated to prevent the surging of the messenger when heav-

Opinions respecting its merit.

* Trans of Soc. of Arts, vol. XXV, p. 65. For this invention the gold medal was voted to Mr. Boswell.

ing

ing in the cable, it certainly possesses great merit, and the idea to me is quite new.

I am, Sir,

Your humble servant,

WILLIAM RULE.

Somerset-place, November 19, 1806.

To Mr. BOSWELL.

SIR,

According to your desire, I transcribe the part of the letter from Mr. Peake (Surveyor of the Navy) to me, which relates to the capstan laid before the Society.

Extract of a Letter from Henry Peake, Esq.

"With regard to your ideas on the capstan; I have tried "all I can to find some objection to it, but confess I "hitherto have been foiled, and shall more readily forward "it, if it was only to supersede a plan now creeping into "the service, more expensive, and much worse than one "lately exploded."

As you and the members of the Committee have seen the letter, I imagine further attestation needless relative to it.

No friction between the turns of the cable or messenger.

I request you will mention, that all friction of the revolutions of the cable (or messenger) in passing each other between the barrels of the capstan, must be effectually prevented by the whole thickness of one of the rings that passes betwixt each crossing. I add this because one of the gentlemen of the Committee wished to be informed on this point.

I am, Sir,

Your very respectful humble servant,

J. W. BOSWELL.

SIR,

In obedience to your intimation, that a written explanation of the advantages to be obtained by the use of capstans made

made according to the model, which I laid before the Society for the Encouragement of Arts, &c., would be acceptable, I send the following, which I hope will make the subject sufficiently clear.

As few but mariners understand the manner in which cables are hauled aboard in large ships, it will probably render the object of my capstan more manifest, to give some account of this operation.—Cables above a certain diameter are too inflexible, to admit of being coiled round a capstan; in ships where cables of so large dimensions are necessary, a smaller cable is employed for this purpose, which is called the *messenger*, the two ends of which are made fast together so as to form an endless rope, which, as the capstan is turned about, revolves round it in unceasing succession, passing on its course to the head of the ship, and again returning to the capstan. To this returning part of the messenger, the great cable is made fast by a number of small ropes, called nippers, placed at regular intervals; these nippers are applied, as the cable enters the hawse hole, and are again removed as it approaches the capstan, after which it is lowered into the cable tier.

Method of hauling large cables on board a ship.

The messenger, or any other rope coiled round the capstan, must descend a space at every revolution, equal to the diameter of the rope or cable used; this circumstance brings the coils in a few turns to the bottom of the capstan, when it can no longer be turned round, till the coils are loosened and raised up to its other extremity, after which the motion proceeds as before. This operation of shifting the place of the coils of the messenger on the capstan is called *surging the messenger*; It always causes considerable delay; and when the messenger chances to slip in changing its position, which sometimes happens, no small danger is incurred by those who are employed about the capstan.

Necessity of surging.

Causes delay and danger.

The first method that I know of, used to prevent the necessity of surging, was by placing a horizontal roller beneath the messenger, where it first entered on the capstan, so supported by a frame, in which it turned on gudgeons, that the messenger in passing over it was compelled to force upwards all the coils above the capstan, as it formed a new coil.

First attempt to obviate this.

This

Disadvantages. This violent forcing of the coils upwards along the barrel of the capstan not only adds considerably to the labour in turning the capstan, but from the great friction which the messenger must suffer in the operation, while pressed so hard against the capstan, (as it must be by the weight of the anchor and strain of the men,) could not but cause a very great wear and injury to the messenger, or other cable wound round the capstan; and that this wear must occasion an expense of no small amount, must be manifest on considering the large sums which the smallest cables used for this purpose cost.

Second attempt. The next method applied to prevent surging was that for which Mr. Plucknet obtained a patent, the specification of which may be seen in the Repertory of Arts, No. 46. In this way a number of upright puppets or lifters, placed round the capstan, were made to rise in succession, as the capstan turned round, by a circular inclined plane placed beneath them, over which their lower extremities moved on friction wheels; and these puppets, as they rose, forced upwards the coils of the messenger on the barrel of the capstan.

This something better. This was a superior method to the first, as the operation of forcing upward the coils was performed more gradually by it; but still the wear of the messenger from the lateral friction in rising against the whelps of the capstan remained undiminished.

Third attempt. The third method used for the same purpose was that proposed by captain Hamilton. It consisted in giving the capstan a conical shape, with an angle so obtuse, that the strain of the messenger forced the coils to ascend along the sloped sides of the barrel. The roller first mentioned was sometimes used with this capstan, of which a full account is inserted in the Repertory of Arts, vol. 2.

Friction as great, and attended with a new inconvenience. The lateral friction, and wear of the messenger against the whelps of the capstan, are equally great in this method as in the others; and it, besides, has the inconvenience of causing the coils to become loose as they ascend; for as the upper part of the barrel is near a third less in diameter than the lower part, the round of the messenger, that tightly embraced the lower part, must exceed the circumference of the upper extremity in the same proportion.

Advantages of In the method of preventing the necessity of surging, which

which the model I have had the honour of laying before the Society represents, none of the lateral friction of the messenger or cable against the whelps of the capstan, (which all the other methods of effecting the same purpose before mentioned labour under,) can possibly take place, and of course the wear of the messenger occasioned thereby will be entirely avoided in it, while it performs its purpose more smoothly, equally, and with a less moving power than any of them. the method now proposed.

My method of preventing the necessity of surging consists in the simple addition of a second smaller barrel or capstan of less dimensions to the large one; beside which it is to be placed in a similar manner, and which need not in general exceed the size of a half-barrel cask. The coils of the messenger are to be passed alternately round the large capstan and this small barrel, but with their direction reversed on the different barrels, so that they may cross each other in the interval between the barrels, in order that they may have the more extensive contact with, and better gripe on each barrel. To keep the coils distinct, and prevent their touching each other in passing from one barrel to the other, projecting rings are fastened round each barrel, at a distance from each other equal to about two diameters of the messenger and the thickness of the ring. These rings should be so fixed on the two barrels, that those on one barrel should be exactly opposite the middle of the intervals between those on the other barrel: and this is the only circumstance, which requires any particular attention in the construction of this capstan. The rings should project about as much as the cable or messenger from the barrels, which may be formed with whelps, and in every other respect, not before mentioned, in the usual manner for capstan barrels, only that I would recommend the whelps to be formed without any inclination inwards at the top, but to stand upright all round, so as to form the body of the capstan in the shape of a polygonal prism, if the intervals between the whelps are filled up, in order that the coils may have equal tension at the top and at the bottom of the barrels, and that the defect which conical barrels cause in this respect may be avoided. This described.

The small barrel should be furnished with falling palls as well as the large one; a fixed iron spindle ascending from the

the deck will be the best for it, as it will take up less room. This spindle may be secured below the deck, so as to bear any strain, as the small barrel need not be much above half the height of the large barrel; the capstan bars can easily pass over it in heaving round, when it is thought fit to use capstan bars on the same deck with the small barrel. As two turns of the messenger round both barrels will be at least equivalent to three turns round the common capstan, it will hardly ever be necessary to use more than four turns round the two barrels.

How lateral friction is prevented.

The circumstance which prevents the lateral friction of the messenger in my double capstan is, that in it each coil is kept distinct from the rest, and must pass on to the second barrel, before it can gain the next elevation on the first, by which no one coil can have any influence in raising or depressing another; and what each separate coil descends in a single revolution, it regains as much as is necessary in its passage between the barrels, where in the air, and free from all contact with any part of the apparatus, it attains higher elevation without a possibility of friction or wear.

May be used for a small cable without a messenger.

Applicable to windlasses.

I have described my double capstan, as it is to be used in large vessels, where messengers are necessary, from the great size of the cables; but it is obvious that it is equally applicable in smaller vessels, as their cables can be managed with it in the same manner as is directed for the messenger. The same principle may also be easily applied to windlasses, by having a small horizontal barrel placed parallel to the body of the windlass, and having both fitted with rings, in the same way as the capstan already described. The proper place for the small horizontal barrel is forward, just before the windlass, and as much below its level as circumstances will admit; it should be furnished with catch-palls as well as the windlass.

Farther advantage stated.

Beside the advantages already stated, my proposed improvement to the capstan has others of considerable utility. Its construction is so very simple, that it is no more liable to derangement or injury than the capstan itself. Its cost can be but small, and every part of it can be made by a common ship carpenter, and be repaired by him at sea if damaged by shot. It will take up but little room, only that of a half barrel

barrel cask; and it is of a nature so analogous to that kind of machinery, to which sailors are accustomed, that it can be readily understood and managed by them.

In order to render the description of my double capstan more clear, I annex a sketch of it, as fitted up in the manner proposed.

I am, Sir,

Your very respectful humble servant,

J. WITLEY BOSWELL.

Reference to the Engraving of Mr. Boswell's improved Capstan, to prevent the necessity of surging. Plate 4, Fig. 5.

A Represents the larger or common capstan used on board ships. Explanation of the plate.

B Another capstan of less dimensions, placed in a similar manner.

C The coils of the messenger passing alternately round the large and small capstans, but with their direction reversed on the different barrels, so that they may cross each other in the interval between them.

D D D D Projecting rings round each capstan or barrel, so fixed on the two barrels, that those on one barrel should be exactly opposite the middle of the intervals between those on the other barrel.

X.

Letter from Dr. Beddoes *on certain Points of History, relative to the Component Parts of the Alkalis, with observations relating to the Composition of the Bodies termed Simple.*

To Mr. NICHOLSON,

Dear Sir,

I Never regarded the base of the alkalis as belonging to the metallic order of combustibles, or projected their reduction Alkalis not supposed by Dr. B. to be

metallic, though substances saturated with oxigen.

duction by galvanism or electricity. But long ago, on contemplating all other substances in opposition to oxigen it very naturally occurred, that, since alkalis and earths would not burn or absorb oxigen, they might be already saturated with it. This investigation, caused by Tondi's paper, would have been, had it operated at all, a discouragement to the idea, which was certainly formed on different grounds, and existed, I believe, prior to my acquaintance with those facts. Such as it was conceived, it happened to be long afterwards thrown out in an essay on the arrangement of bodies on the principle alluded to above. As a distinct fourth class of bodies I had arranged together barytes, strontites, potash, soda, lime, magnesia, alumine, jargonites, silex, &c., adding this query "Does the mode of union of their elements render them nonoxidable? or have they already oxigen or pho. oxigen closely combined?" and again "If future experiments should accomplish the oxidation of any of the bodies of the fourth class, such bodies must be transferred to the third class (termed philoxigenous). Should it be discovered, that oxigen enters into their composition, the terms philoxigenous and misoxigenous must be changed*."

Arranged with certain earths as a distinct class of bodies.

Query respecting them.

Analysis of bodies termed simple suggested.

I had observed, p. 218, that, "more than mere classification, I had it in view to place under the reader's eye certain probabilities, that might lead to the analysis of different bodies, at present considered as simple." This application of electricity is a project, which has lain on the surface of chemistry for above twenty years. I have taken all opportunities, public and private, of pressing its execution. The bodies I have been accustomed to name as the proper subjects for trial were the fusible combustibles, as sulphur and phosphorus. A gentleman, illustrious for his late success in these researches, some time ago mentioned to me his having made this experiment with galvanism. The result was the liberation of some vapours or gas, which disappeared again before the body congealed. The mode of investigation should, in my opinion, still be prosecuted with a much higher power than has yet been employed.

Electricity long considered as the proper mean.

The fusible combustibles proper for this.

Tried by Mr. Davy.

A gas liberated, and again absorbed.

Metals and other combustibles may be

As an incentive and a clew to experiment (which is the only use of hypothesis) I beg leave to repeat, that *metals*

* Contributions to phys. and med. Knowledge, p. 223.

and

and other combustibles may be formed of hidrogen and azote. The opinion has gained some countenance from the analogy between volatile and fixed alkalis, together with the identification of the base of the fixed with metals. The reported amalgamation of the base of volatile alkali with quicksilver is an important link in the same chain of ideas; though the amalgamation of charcoal with iron, &c. may be opposed, unless charcoal prove a metallic oxide or hidrogenate.

formed of hidrogen and nitrogen.

Some confirmations of this.

Is charcoal metallic?

One cannot proceed far in this train of speculation without getting the prospect of all nature as consisting of two elements, oxigen and hidrogen.

In respect to heat, light, electricity, galvanism, and magnetism, I see not the smallest reason to regard these as distinct substances, or other than as powers or influences, if we are not to follow Berkeley. We have no right to consider any property whatever as essential to matter. We have therefore no criterion of materiality. Yet it appears to me, that the absence of gravitation is a much stronger negative argument than any positive yet produced: and I have no doubt but all those who have set themselves to weigh *caloric*, under the notion of its being a separate substance, have been miserably disappointed at the result of their experiment; and that, had the result been opposite, they would have triumphed, and justly, in this proof; for it must have been received as decisive. Have not adversaries a right to retaliate?

Heat, light, electricity, galvanism, magnetism, not proved to be any thing but powers.

Perhaps gravitation the only criterion of matter.

The genius of accurate experimental investigation may be now in the art of striding from inanimate to living nature; very soon afterward one may venture to predict, that other influences, offering other means of analysis, will be discovered, less extensive probably than heat, and more so than magnetism, and constituting the difference between the particles of matter as they happen to be engaged in one class of compounds or the other. The Archaeus, vital principle, Mr. Hunter's materia vitae diffusa, &c., will perhaps come to be considered as anticipations (clumsy and illogical ones indeed) of such influences.

We may be on the eve of attaining a knowledge of animate nature.

Other influences, affording other means of analysis, to be discovered.

Anticipations of these.

I am, dear Sir, Yours respectfully,

10*th Sept.* 1808. THOMAS BEDDOES.

XI.

XI.

Analysis of some metallic Sulphurets. By Mr. GUENIVEAU, *Mine Engineer*.*

Proportion of the metal in sulphurets constant.

SEVERAL chemists, particularly Messrs. Proust and Hatchett, have paid attention to metallic sulphurets. The first of these gentlemen has shown, that certain metals, as iron, copper, and lead, combine with sulphur in the metallic state and in a constant proportion. Mr. Hatchett has given an analysis of the magnetic pyrites, which he considers as a sulphuret of iron at a minimum, and that of several common pyrites, in which he finds other principles beside iron and sulphur. The experiments of these two scientific gentlemen however have not impressed conviction on the mind of every chemist; and some appear still to admit the presence of oxigen in sulphurets of iron. They found their objections chiefly on this, that Mr. Proust employed the method of synthesis, which always leaves some uncertainty in the proportions: and that Mr. Hatchett ascertained the sulphur only by means of sulphate of barytes, respecting the composition of which some uncertainty still remains. Having had occasion to analyse certain metallic sulphurets, I determined their elements with a great deal of care, in order to satisfy myself on the points just mentioned.

Magnetic and other pyrites.

Some still admit the presence of oxigen.

Various specimens examined.

Sulphuret of iron.

The specimen of sulphuret of iron, on which I made all the experiments I am about to describe, was amorphous, without any mixture of gangue. Its colour was the common bronze yellow of iron pyrites. Various preliminary experiments convinced me, that this mineral contained no earthy substance, and no other metal than iron. I shall now proceed to describe the methods I employed to determine with precision the quantities of iron and sulphur they contained.

* Journal des Mines, vol. XXI, p. 105. A translation of a paper by Mr. Gueniveau on the Desulphuration of Metals, in the same work, was given in our Journal for November last, vol. XVIII, p. 197.

I. Examination

I. *Examination for the iron.*

1. I boiled a mixture of nitric and muriatic acids on five grammes [77 grs.] of powdered pyrites. The sulphur was completely burned, and the solution was complete, except 0·01 of a gramme of silex. The oxide of iron precipitated by ammonia and heated red hot weighed 3·35 gr.: which indicate, supposing the proportion to be 148 to 100, 2·25 gr. of metallic iron, or 45 per cent. Analysed. Boiled in aqua regia. Precipitated by ammonia.

2. Another experiment made in the same manner yielded me 3·34 gr. of red oxide of iron; which coincides with the preceding. This repeated.

3. I roasted 20 gr. [308 grs.] of the same pyrites. After being exposed some hours to a pretty violent heat, the weight was reduced to 13·24 gr.: so that 100 left only 66·2. Roasted.

Of this residuum I dissolved 5 gr. in nitromuriatic acid. Muriate of barytes producing no precipitate in this solution, I concluded, that the roasting had been complete, and the pyrites reduced to pure oxide of iron. Besides, on comparing the weight of the residuum of 5 gr. of pyrites, being 3·31 gr., with that of the oxide of iron obtained by the experiment above, namely 3·34 gr., there can be no doubt, but the whole of the sulphur and sulphuric acid were volatilized. This new method of computing the quantity of oxide of iron leaves no doubt respecting the proportion of metal in the pyrites, being equally indicative of 45 per cent of metallic iron. Residuum dissolved in aqua regia. No precipitate with muriate of barytes.

4. I fused the roasted pyrites without any addition in a crucible lined with charcoal, in order to obtain the metal. The button amounted to 70·2 per cent, without any scoriæ. Deducting 3 per cent for the carbon combined with it, we shall have 68·1 of iron from 100 of roasted pyrites, and from 100 of pyrites in its native state 45·08 of pure iron. Roasted pyrites fused without addition.

From the four experiments here mentioned it follows, that the sulphuret of iron contains 45 hundredths of metallic iron; and I do not think, that any errour can have taken place to the amount of one hundredth. Contained 0·45 of metallic iron.

II. *Examination*

II. *Examination for the sulphur.*

Dissolved in aqua regia and precipitated by muriate of barytes to ascertain the quantity of sulphur.

1. Having dissolved 5 gr. of iron pyrites in nitromuriatic acid with the assistance of heat, I dropped into the solution muriate of barytes, till no more precipitate was formed. The sulphate of barytes subsided to the bottom of the vessel; and, having poured off the clear liquor, I added some distilled water, in order to wash off any foreign salts. I collected the sulphate on a filter. Having dried it, first with a gentle heat, increased afterward to redness, and burned the filter separately, I found the weight of the sulphate of barytes, deducting that of the ashes of the filter, was 19·1 gr., or 382 to 100 of pyrites.

2. It might be suspected, that the preceding result was too small, on account of the state of ebullition in which I had kept the solvent, which might have carried off in vapour a portion of the sulphuric acid formed. I thought it right therefore, to make another experiment, employing a more moderate heat.

Treated with dilute nitric acid in a gentle heat.

Accordingly I treated 2·5 gr. of the same pyrites with diluted nitric acid, heating it gently. The whole of the sulphur however was burned except about 0·03 of a gramme that remained undecomposed. From this solution I obtained 9·71 gr., or 388 per cent of sulphur of barytes, corresponding to 54·3 of sulphur; and, on taking into the account the residuum abovementioned, we shall have 54·8 of sulphur in a hundred parts.

This result I consider as more accurate than the preceding.

0·55 of sulphur.

The experiments I have related clearly show, that the sulphuret of iron analysed contained about 45 per cent of metallic iron, and between 54 and 55 per cent of sulphur, results which differ very little from those of Mr. Hatchett. It is difficult then to conceive, that iron pyrites contain oxigen, and the quantity corresponding to all the mistakes that could possibly have taken place cannot be many hundredths.

Component

Component parts of iron pyrites.

Metallic iron	45
Sulphur	55
	100.

Sulphuret of copper.

Messrs. Lelievre and Gillet-Laumont, mine-counsellors, having had the goodness each to present me with a specimen of sulphuret of copper, I shall proceed to give the results of my analysis of this mineral. Sulphuret of copper.

1. Siberian sulphuret of copper from the collection of Mr. Lelievre. Spec. gravity 5·22.

Five grammes of this mineral treated with nitromuriatic acid assisted by heat were reduced to 0·51 of a gr. of sulphur nearly pure. Calcination left only 0·04 gr. of oxide, which was completely redissolved. Treated with aqua regia.

The solution precipitated by muriate of barytes let fall 4·01 gr. of sulphate, corresponding to 0·56 of a gr. of sulphur. This brings the whole quantity of sulphur to 1·03 gr. The iron was separated from the copper by ammonia. The precipitate, well washed and dried, weighed 0·08 of a gr. Precipitated by muriate of barytes.

The brown oxide of copper precipitated by potash weighed 4·65 gr., answering to 3·72 gr. of metallic copper.

I convinced myself by various experiments, that the specimen subjected to analysis contained no earthy substance, lead, manganese, or antimony. The small quantity of iron existing in it appeared to me even to be included in small fissures, in which its oxide is easy to be perceived: it cannot therefore be considered as an essential part of the composition of sulphuret of copper. No earth, lead, manganese, or antimony. A little oxide of iron foreign to it.

The results of this analysis are

Metallic copper	74·5
Sulphur	20·5
Oxide of iron	1·5
Loss	3·5
	100.

Component parts.

The experiments I made in the course of the analysis lead me to think, that part of the loss fell on the copper. Notwithstanding this however, the proportion of sulphur to copper

copper differs very little from that of 28 to 100 given by Mr. Proust.

Fused in charcoal.

This sulphuret of copper, being subjected to a very violent fire in a crucible lined with charcoal, was fused, and lost but 2½ per cent of its weight. Its aspect was not altered, only a few small globules of copper were perceptible toward the bottom of the button.

2. *Siberian sulphuret of copper from the collection of Mr. Gillet-Laumont.*

Another specimen mixed with quartz.

This specimen, though in appearance very homogeneous, was notwithstanding mixed with a great deal of quartz. In some places it struck fire with steel.

Analysis.

I separated the copper from the iron by sulphuretted hidrogen. The precipitate, calcined, redissolved, and treated with caustic potash gave me 3 gr. of oxide of copper from 5 of the mineral. I found in it no other metal but copper and iron.

The results were

Component parts.

Metallic copper	47
Sulphur	13
Silliceous residuum	25
Lime	7
Red oxide of iron	9·3
	101·3

Proportions of the sulphur & metal not affected by foreign substances.

It is to be observed here, that the presence of the different substances foreign to the sulphuret of copper did not affect the proportions of the copper to the sulphur, which is evidently that of 100 to 28. The iron probably is not combined with the sulphuret, but with the silex and lime forms its gangue.

Copper pyrites.

Pyritous copper.

I. Copper pyrites of Sainbel from the collection of the Council of Mines. Spec. gravity 4·16.

The specimen I subjected to analysis was amorphous, but without mixture of gangue. Its colour was a greenish yellow bronze. I ascertained its composition in two different methods.

1st

1st analysis. Five grammes of this mineral, powdered, and treated with nitromuriatic acid, were very easily attacked by it. The residuum, weighing 1·13 gr., was reduced to 0.08 of a gr. by calcination; and an addition of fresh acid left only 0·04 of a gr. of quartzose gangue. Treated with aqua regia.

Muriate of barytes threw down from the solution a precipitate of sulphate weighing 5·5 gr., corresponding to 0·77 of a gr. of sulphur. This quantity, added to that already formed, gives 1·82 gr. for the whole of the sulphur it contained. The copper was precipitated by sulphuretted hidrogen, redissolved, and precipitated afresh by caustic potash. The brown oxide obtained weighed 1·88 gr., and contained nearly 1·5 gr. of metal. Precipitated by muriate of barytes, and then by sulphuretted hidrogen.

The potash had dissolved about 0·05 of a gr. of oxide of zinc. The red oxide of iron weighed 2·26 gr., corresponding to 1·53 of metallic iron.

Results.

Component parts.

Sulphur	36·5
Copper	30
Metallic iron	31
Oxide of zinc	1
Gangue	1
	99·5

2d analysis. The same substance was treated with nitric acid assisted by heat. Treated with nitric acid.

First residuum, 2·35 gr. reduced to 1·86 gr. by calcination. The nitromuriatic acid left of this only 0·23 of a gr., containing only 0·04 of a gr. of gangue. Residuum calcined and treated with aqua regia.

The weight of the sulphur separated from these residuums was 0·93 of a gr. The solution gave a precipitate of 5·91 of sulphate of barytes, containing 0·82 of a gr. of sulphur, and making the whole amount to 1·75 gr. Precipitated by muriate of barytes.

The copper was dissolved by ammonia, and the oxide of iron separated from it by several operations. The oxide of copper precipitated by potash weighed 1·9 gr., answering to 1·52 gr. of the metal. Copper dissolved by ammonia, and iron separated.

The red oxide of iron weighed 2·47 gr., and contained 1·66 gr. of pure iron.

I likewise found traces of zinc.

 Result.

Result.

Component parts.

Sulphur	35
Copper	30·5
Metallic iron	33
Some traces of zinc	
Gangue	1
	99·5

If we take a mean of the results of these two analyses, we shall have as very probable proportions.

Mean of the two analyses.

Sulphur	36
Copper	30
Metallic iron	32
Gangue	1
Zinc	1
	100

Another specimen.

II. Copper pyrites of Baigorry.

For the two following analyses I employed pieces of ore that were sufficiently pure, though mixed with quartz.

Treated with aqua regia.

1st analysis. Five grammes reduced to powder were subjected to the action of nitromuriatic acid. The first residuum, weighing 1.72 gr., was reduced to 0·73 of a gr. by calcination. An addition of acid left only 0·54 of a gr., and of these 0·46 were found to be gangue, after the sulphur had been burned.

Precipitated by muriate of barytes,

The muriate of barytes precipitated from the solution 3·6 gr. of sulphate, corresponding to 0·5 of a gr. of sulphur. The whole of the sulphur therefore was 1·57 gr.

and sulphuretted hidrogen.

Sulphuretted hidrogen was employed to separate the copper. The brown oxide of this metal, precipitated by potash, weighed 1·69 gr., and consequently contained 1·35 of metal. The red oxide of iron obtained weighed 2·19 gr., corresponding to 1·49 gr. of metallic iron.

Result.

Component parts.

Sulphur	31·5
Copper	27
Metallic iron	30
Gangue	8·5
	97

Analysis repeated.

2d analysis. I treated 5 gr. of the same substance in the

same

same way. I separated by calcination 0·34 of a gr. of sulphur. The gangue weighed 0·48 of a gr. The sulphate of barytes obtained weighed 8·88 gr., corresponding to 1·24 gr. of sulphur. The whole of the sulphur therefore was 1·58 gr.

The brown oxide of copper weighed 1·73 gr.; the red oxide of iron, 2·16 gr.

Result.

Component parts.

Sulphur	31·5
Copper	28
Metallic iron	29
Gangue	9
	97·5

Mean proportions.

Mean of the two analyses.

Sulphur	31·5
Copper	27·5
Metallic iron	29·5
Gangue	9
	97·5

Sulphur not completely ascertained, I have reason to think, that the proportions of sulphur are rather too small, because all the methods employed never give the whole of this combustible.

and generally comes out less than the truth. When metallic sulphurets are treated with nitric acid diluted in water, the sulphur remains mixed with the metals, which become oxided during the evaporation. All the oxigen added diminishes the quantity of the sulphur. By employing nitromuriatic acid and boiling, this inconvenience is avoided; but sulphuric acid may be carried of in vapour. Whatever method we adopt, the quantity of sulphur obtained may always be considered as below what really exists.

Proportions of sulphur, copper, and iron nearly uniform. Notwithstanding the errours unavoidable in analyses, it is easy to perceive, that the relative quantities of sulphur, copper, and iron, are nearly the same in the two specimens of copper pyrites. Setting aside the gangue, and reducing the proportions to hundredth parts of pure ore, we find

	Sulphur.	Copper.	Iron.
In the copper pyrites of Sainbel	37	30.2	32·3
In that of Baigorry	35	30·5	33

A mixture of the two sulphurets. Mr. Proust has shown, that the copper pyrites contains sulphuret of copper completely formed, and he considers this mineral as a mixture of the two sulphurets of copper

and

and iron. This opinion appears to me very probable: though perhaps we have not sufficient grounds to assert, that the sulphuret of iron exists in it in the same state of combination as that, which constitutes native iron pyrites.

Analysis of Mr. Chenevix nearly similar.

Mr. Chenevix obtained from a specimen of copper pyrites 30 per cent of copper, and 53 of oxide of iron, corresponding to 35 of the metal. I likewise found 30 per cent of copper in a piece of yellow ore from Chessy. On comparing these results with those above given, I was struck with the proportion that exists between the elements of a mineral generally considered as varying greatly in its composition. The difficulty of distinguishing it from iron pyrites may have contributed to this opinion: but I am inclined to think, that, when copper pyrites is completely homogeneous, and not decomposed, its composition is the same, from whatever ore it be obtained; and that it may be considered as a mineralogical species, ascertained and determined by chemistry.

This confirms the idea of its uniformity.

This however is but a simple conjecture, on which nothing positive can be said, till we have a greater number of analyses made on well marked specimens free from any mixture.

XII.

Analysis of a Carbonate of Lime from Pesey; by Mr. Berthier, *Mine Engineer*.*

The carbonate described.

THE carbonate of lime from the mine of Pesey is found in geodes grouped comfusedly with quartz, and sometimes with lenticular polishing spar. Its specific gravity is 2·97.

Its figure is that of the primitive rhomboid of common carbonate of lime. It may be split with great facility, and divides in the direction of its longer diagonals. All the faces are covered with striæ in this direction.

Its hardness is much greater than that of common carbonate of lime, which it scratches. It even scratches arragonite. The pieces found on the heaps of rubbish, that have remained long exposed to the air, have the brown co-

* Annales de Chimie, vol. LVIII, p. 87.

lour

lour of iron spar. It is sometimes perfectly transparent: but most commonly it has a slight yellowish tint, becomes opake, and is covered with a brown oxide, as it is decomposed in the damp parts of the mine.

Before the blowpipe it becomes black, and is slightly altered. It scarcely effervesces with acids, unless previously powdered.

Having powdered and sifted it, I took 5 grammes [77 grains], on which I poured strong nitric acid. On applying a gentle heat, the effervescence immediately became very brisk; nitrous gas was evolved; and the powder assumed a brown colour. Having evaporated to dryness, I poured on fresh acid, and repeated the same operation. Analysed. Treated with nitric acid.

I next dissolved the whole in muriatic acid, evaporated gently to expel the excess of acid, and then dissolved in water. The solution, which was of a light yellow, was precipitated by prussiate of potash. The result was a deep blue prussiate, which was filtered and washed. With muriatic, & precipitated by prussiate of potash,

The solution, completely neutralized, was precipitated by oxalate of ammonia, and yielded oxalate of lime, which, when washed and dried, weighed 3·95 grammes. and by oxalate of ammonia.

Caustic potash threw down from the liquor a copious white flocculent precipitate, which, when washed, dried, and calcined in a red heat, weighed 0·5 of a gr. This substance, which was of a fine white colour, dissolved entirely in sulphuric acid, and yielded a bitter salt. Carbonate of ammonia did not precipitate it; therefore it was magnesia. Caustic potash threw down magnesia.

The prussiates when dried were strongly calcined, and the residuum oxided to a maximum by nitric acid. Iron oxided,

The oxides were redissolved in muriatic acid; evaporated gently to neutralize them; and then diluted with a large quantity of water. No residuum was left. dissolved in muriatic acid,

This solution was precipitated by carbonate of potash, saturated, and afterward filtered. precipitated by potash,

The carbonate deposited on the filter was redissolved in muriatic acid, precipitated afresh by carbonate of potash, saturated, and filtered. redissolved and precipitated.

The two filtered liquors being mixed together, they were subjected to evaporation for about two hours, when they deposited Liquors evaporated

deposited a slightly yellowish white substance, which, when washed and dried, weighed 0·16. Before the blowpipe it immediately became black, and communicated a violet colour to borax. In nitric acid it redissolved with effervescence; the nitrate grew black on drying; and the residuum, treated with muriatic acid, gave out oxigenized muriatic acid, and produced a brown colour, which heat removed. Lastly, prussiate of potash threw down from the muriatic solution a white precipitate, without any perceptible mixture of blue. Thus there can be no doubt, that the carbonate deposited by ebullition was carbonate of manganese.

deposited manganese.

Iron left on the filter.

The supernatant liquor contained nothing more. The carbonate remaining on the filter was red. This was dissolved in muriatic acid, and precipitated by prussiate of potash, which produced a blue prussiate of iron, weighing, when well washed and dried, 1·9.

The carbonate calcined.

Five grammes of the carbonate were calcined in a crucible at a red heat, and lost from 1·8 to 1·85 of water and carbonic acid.

This substance therefore contained, in 100 parts,

Component parts.

Lime	43·5
Magnesia	10
Black oxide of iron	8
White oxide of manganese	2
Water and carbonic acid	36·5
	100.

A compound of four carbonates.

Thus the four carbonates of lime, magnesia, iron, and manganese, which sometimes occur separately in nature, are found united together in the substance, of which an analysis has just been given. I do not think there can be any doubt of the presence of the manganese. In one experiment I found four per cent of this metal in the state of white oxide: but I have preferred giving the proportion above, which consequently is a minimum.

These carbonates variously compounded.

If the results here related be exact, we may conclude, that the carbonates of lime, magnesia, iron, and manganese, may be found in nature in various proportions; so that it is no wonder we meet with iron spars without manganese, and others mixed with manganese alone, without lime or magnesia,

nesia. The analyses of these substances become so much the more interesting to the metallurgist, and we see clearly one source of the difference, that may exist between iron spars.

XIII.

Chemical Examination of the Stalk of Indian Corn, Zea Mays Lin., to ascertain whether the Saccharine Matter it contains be capable of Crystallization; by Mr. V. AUARIE, *Apothecary at Valence, Department of the Drôme*.*

IF it can be said with truth, that our physical knowledge of vegetables is a complete science, their analysis individually is far from having attained this desirable end. The labours of modern chemists however are paving the way for it; their numerous scientific discoveries have already illustrated this subject, of so much importance to the art of pharmacy; and other arts, as well as that of physic, are daily availing themselves of it with success. Still we have to regret, that the analysis of vegetables is most uncertain, since the results are too often far from satisfactory, and the synthetical method is in many cases impracticable. Analysis of vegetables far from perfect.

I. I boiled in a sufficient quantity of water fifteen pounds of the stalks of Indian corn, freed from their leaves and roots, and previously bruised. The decoction after it was filtered was of a golden yellow colour, and a saccharine taste. Part of this decoction was subjected to various experiments, of which the following were the results. Stalks of Indian corn boiled in water. The decoction tested with

1. A solution of crystallized acetite of lead rendered the decoction turbid, and separated its colouring and extractive part. These had subsided to the bottom of the vessel in the course of an hour, leaving the liquor very clear, and lighter coloured. acetite of lead,

2. The acidulous oxalate of potash produced a sediment, and left the liquor milky. oxalate of potash,

* Annales de Chimie, vol. LX, p. 61. Some remarks on the charcoal both of the stalks and seeds of maize, by prof. Proust, were inserted in our Journal, vol. XVIII, p. 239.

3. A

acetite of ammonia, 3. A solution of acetite of ammonia heightened its colour, and produced a scarcely perceptible precipitate.

muriatic and oximuriatic acid, 4. Common muriatic acid occasioned no change: but oxigenized muriatic acid produced a slight precipitate, without altering the colour of the liquor. This precipitate was occasioned by the oxigen, which attacked the extractive matter, and rendered it insoluble in water.

potash, 5. Caustic potash produced no change.

lime-water, 6. Lime-water and prussiate of lime rendered it slightly turbid.

sulphate of iron, 7. Sulphate of iron had no effect on it, not even altering its transparency.

sulphuret of potash, 8. It was the same with sulphuret of potash.

nitrate of mercury, 9. Nitrate of mercury was decomposed, and formed a coagulum, which subsided to the bottom of the vessel and was of a deep gray colour.

and alcohol. 10. Alcohol produced no satisfactory result.

Liquor still saccharine. It is to be observed, that none of the reagents employed either destroyed or altered the saccharine taste, which continued in the liquor; nothing being decomposed and precipitated but the gummy extract.

Decoction evaporated & left at rest, but no crystals. II. The greater part of the decoction was evaporated to the consistence of a sirup, and afterward set by. Having been left undisturbed for twenty days at a temperature of 10° [50° F.], it was just the same, without any appearance of crystals or sediment.

Diluted, clarified, and again left at rest. No crystals. III. Having diluted it with twenty times its bulk of water, clarified it afresh, to separate the mucilage that appeared to prevent its crystallization, and reduced it to a syrupy consistence, I was not more successful, after leaving it a proper time at rest.

Evaporated & digested in alcohol. It was of importance therefore, to separate the extractive matter from the saccharine part. Accordingly I evaporated the decoction, thus clarified a second time, to the consistence of an extract, and digested it in a sufficient quantity of alcohol. Twenty-four hours after I filtered it, when the alcohol had dissolved half the matter subjected to its action.

Solution. The alcoholic solution had the taste of a very sweet dram, except that it had no aromatic flavour. Its colour was a brown yellow.

This

This solution mixed with water underwent no change, remaining perfectly clear; which is a proof, that the alcohol had dissolved no resin, and that the saccharine matter only was in the solution. No resin.

After having separated the alcohol in the common way, the substance remained fixed, and would not crystallize. It comported itself like the treacle of the shops. Not crysallizable.

Not being by any means satisfied with the results above mentioned, I again diluted the sirupy matter, that had been dissolved by the alcohol, with a sufficient quantity of water; I added a little lime and white of egg in the clarification; and after filtering and evaporating to a due degree I set it by for two months in a stove, but without obtaining any crystals. Clarified with lime and albumen. No crystals.

I employed successively all the processes employed in sugar-houses, without any success. I carried my experiments so far as to boil it with charcoal, and after I had clarified it, I was equally unsuccessful. It retained, and still retains, for I have left it to the effect of time, its honeylike appearance; yet it possesses all the other characters of the true sugar extracted from the sugar-cane of the West Indies. Various methods tried without success.

IV. The substance that remained insoluble in the alcohol was completely dissolved in distilled water. Its taste was saponaceous and slightly saccharine. After evaporating to the consistence of an extract, it weighed four ounces and half. One ounce of this was treated with nitric acid, which dissolved it in the same manner as it would have done a gum. During the solution a great deal of nitrous gas was evolved, and at the same time oxalic acid was formed. Matter insoluble in alcohol. Treated with nitric acid comported itself as gum.

The remaining three ounces and half of extract were afterward incinerated. During the combustion a large quantity of carbonic acid was given out; the matter swelled up, so that the coal was twenty times the original bulk, and very porous; the residuum, after incineration, weighed half an ounce; and this, when dissolved, filtered, and evaporated, was reduced to two drams. Incinerated.

I found by the processes I employed, that the salt resulting from these operations was carbonate of potash with a little magnesia. Carbonate of potash and a little magnesia.

From all that has been said above it follows:

1st, That the stalk of Indian corn cannot be employed for General conclusions.

for the extraction of sugar, because the expense would exceed the profit, since a hundred weight yields only about two pounds of saccharine matter.

2d, That the saccharine matter constantly retains the consistence of treacle, and is incapable of being crystallized by any known process.

3d, That the gummy extract might be employed in medicine as an attenuant, in consequence of its saponaceous quality.

XIV.

On the Culture of Spring Wheat, and the Use of Tincture of Opium in the Diseases of Cattle: by Major SPENCER COCHRANE, *of Muirfield-House, near Haddington, North-Britain**.

SIR,

I REQUEST the favour that you will present my thanks to the Society of Arts, &c. for transmitting me the 20th volume of their Transactions, containing my former experiments on the culture of wheat sown in the spring. I have since had further proofs of the advantage resulting from that practice.

Advantages of spring wheat.

Notwithstanding the extreme cold weather, which we had here during the months of March, April, and May, I never saw my spring wheat look better, particularly four acres, part of a field of strong clay, which I was prevented from sowing in October, when I sowed my other wheat, by wet weather commencing.

The whole field consists of twenty acres, and received one ploughing after drilled beans. I sowed on the 14th of March the four acres, with four bolls or two quarters of common wheat, on the same earth or furrow which the land got in the month of October.

Should be sown on winter furrow.

In the event of land having been fallowed and sufficiently cleaned before winter, and wet weather setting in so as to prevent wheat being sown at the usual time, I recommend from experience, that the wheat be sown in the spring on the win-

* Trans. of Soc. of Arts, vol. xxv. p. 29. The silver medal of the Society was voted to Major Cochrane.

ter

ter furrow, and that it should by no means be ploughed again in the spring.

The winter frost meliorates the soil, and I think kills the annual weeds. I have remarked that by adopting this mode, the land is much less troubled with them, the weeds having been a general objection to spring wheat.

If spring wheat follows turnips, the ground should be ploughed as soon as possible, if the soil is of a wet nature, to correct the injury the land may have sustained by leading off the crop, and by the poaching of carts and horses. Frost will in some degree correct what should never if possible happen, *wet ploughing*.

From the middle of February till the 10th of March is the proper time for sowing wheat in the spring, provided the land is sufficiently dry. Then on the first furrow let the seed properly pickled be sown either by drill or broad cast; the usual practice of water furrowing, to keep the land from too much rain, being properly attended to. Proper time for sowing.

On the Use of Tar for Cattle swelled by eating Clover.

Cows are frequently seized with violent swellings from having been imprudently allowed to eat clover when wet. A gentleman recommended to me, as a cure, an egg-shell full of tar, immediately to be put down the creature's throat. In two instances of my own cattle I found it had the effect of laying the swelling in a few minutes. A neighbour of mine, whose cow it was supposed could not live five minutes, was, on application of the tar, unexpectedly recovered, to the great joy of the poor man. Tar cures cattle swelled with eating clover.

On Opium and other Preparations from Poppies.

After I commenced farmer, I unfortunately lost four horses, by a disorder very frequent in this country, called the *bats* or *gripes*; some of them died in a few hours, and none of them were ill more than two days. For some years past I have given my horses in such cases a table spoonful of tincture of opium, or liquid laudanum, and have since lost none. If the first dose given in some liquid does not allay the violent pain and swelling, I administer a second spoonful, which I have hitherto, in all cases, found to have the desired effect, and generally in a very short time. Opium a cure for the gripes in horses.

If

If I find the horse very hot and feverifh, and sweating profusely, as is usually the case in this disease, I order him to be bled plentifully, and an ounce or more of nitre to be mixed, and administered with the laudanum, keeping the horse warm, and letting him be well rubbed round the belly.

A very considerable farmer near me, who has had a medical education, told me, a few days ago, that he had not lost a horse since he gave them laudanum.

Equally useful to sheep, with common salt.

Ten days ago I was equally fortunate in a trial of it on one of my sheep, which, half an hour after being washed with the rest of the flock, was taken so extremely ill, and swelled so much, that my herdsman supposed she could not live, having lost some of his own, which had apparently been in the same state. I immediately ordered half a handful of common salt to be dissolved in half an English pint of warm water, into which I put sixty drops of the laudanum, and poured it with difficulty down the animal's throat, which seemed nearly dead. For the first five minutes I had so little hopes of the sheep's recovery, that I ordered the man to get his knife ready to cut her throat; whilft he sharpened the instrument for such purpose, he observed the animal to move his jaw to a proper position, which had previously been much distorted; the eyes then began to quicken, and apparently to become at ease. In half an hour afterwards the sheep got on her legs, and remained standing for some time; a plentiful evacuation soon took place, the swelling subsided, she continued to recover, and in a few hours from the first attack began to eat and do well.

My intent in these communications is to render generally public what I have found so very beneficial. At this time, when horses and cattle are so extremely high in price, every thing that can tend to preserve their lives, fhould be made known and put to trial.

Poppies cultivated for the opium.

I formerly noticed to you, that I had tried on a small scale, for several years, the culture of white poppies to prepare opium from them, and an extract or syrup of poppies: that I had raised a sufficient supply for myself and friends, and that my extract was equal in effect to any prepared from foreign

foreign opium. I recommend the poppy seeds to be sown in March, in drills.

Beside the advantages from the poppy heads as a medicine, the seeds yield a valuable oil. Two pounds of the seeds furnish by expression seven ounces of a pure bland oil, useful for portrait painting and other purposes. It has been proved in Holland to be equal in quality to fine salad or olive oil, and it would probably be advantageous to propagate largely so valuable a plant*. and for oil.

I am, Sir, your humble servant,

SPENCER COCHRANE.

SCIENTIFIC NEWS.

Decomposition of the Earths.

IN a paper lately read before the Royal Society Mr. Davy has detailed a number of experiments, made by means of Voltaic electricity, on the common and alkaline earths, by which he has succeeded in effecting their decomposition, and obtaining metals from most of these refractory bodies. Metals obtained from most of the earths.

His method of decomposing the alkaline earths is by electrifying mixtures of them and metallic oxides, such as those of quicksilver, silver, and tin. The common metals and the metals of the earths are revived together in alloy. Revived in alloys by mixing with metallic oxides.

He has succeeded in obtaining the pure metals of barytes and strontites, by distilling their amalgams: and in the same way has procured the metals of lime and of magnesia nearly pure. Bases of barytes & strontites. Lime and magnesia.

He has obtained marks of the decomposition of alumine and silex, by electrifying mixtures of these earths and potash, but has not yet succeeded in obtaining their metals pure. Alumine and silex.

Mr. Davy has repeated a remarkable experiment of Messrs. Berzelius and Pontin, of Stockholm, from which it appears, that hidrogen and nitrogen are capable of combining with quicksilver, and of forming with it a metallic amalgam, which by oxidation produces ammonia. Hidrogen and nitrogen form an amalgam with mercury.

Mr. George Singer will commence his lectures at the Scientific Institution, No. 3, Princes Street, Cavendish Square, early in November, with an extensive course, on the nature, use, and properties, of the atmosphere, a historical sketch of the progress of atmospherical discovery, and an experimental elucidation of every interesting phenomenon dependent on the agency of air, including pneumatics, hydrostatics, natural chemistry, and meteorology, illustrated by an extensive and appropriate apparatus. Lectures on the nature, use, & properties of air.

* On this subject see our Journal, vol. XIX, p. 282.

METEOROLOGICAL JOURNAL

For SEPTEMBER 1808,

Kept by ROBERT BANCKS, Mathematical Instrument Maker, in the STRAND, LONDON.

AUG. Day of	THERMOMETER. 9 A. M.	9 P. M.	Highest.	Lowest.	BAROMETER.	WEATHER. Night.	Day.
27	61	61	71	52	29·63	Fair	Fair
28	52	57	67	54	29·74	Ditto	Ditto
29	59	62	69	60	29·93	Ditto	Ditto
30	57	66	72	60	29·77	Ditto	Rain
31	63	60	68	59	29·64	Ditto	Ditto
SEPT.							
1	62	59	67	55	29·66	Cloudy	Rain
2	61	56	65	55	29·84	Ditto	Ditto
3	58	56	65	49	29·86	Fair	Ditto
4	62	57	64	53	29·83	Ditto	Ditto
5	61	57	64	55	29·80	Ditto	Ditto
6	59	58	63	54	29·77	Rain	Ditto
7	58	60	64	57	29·82	Ditto	Ditto
8	61	57	66	55	29·64	Fair	Ditto
9	57	59	62	54	29·38	Rain	Ditto
10	56	57	60	53	29·37	Fair*	Ditto
11	56	56	60	54	29·51	Ditto	Ditto †
12	55	56	59	54	29·66	Cloudy	Ditto
13	56	58	61	51	29·74	Ditto	Ditto
14	57	58	63	57	29·82	Ditto	Fair
15	62	60	68	56	30·05	Fair	Ditto
16	61	57	65	52	30·26	Ditto	Ditto
17	60	58	64	54	30·22	Ditto	Ditto
18	59	61	66	58	29·98	Ditto	Rain
19	61	59	65	53	29·99	Ditto	Ditto
20	58	58	66	50	30·23	Ditto	Fair
21	57	57	64	52	30·31	Ditto	Ditto
22	56	60	68	52	30·15	Cloudy	Rain
23	56	50	64	44	29·71	Fair	Ditto
24	48	51	54	48	29·99	Ditto	Fair
25	52	54	57	49	30·08	Ditto	Ditto

* Foggy at midnight. † Thunder at 11 A. M.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

NOVEMBER, 1808.

ARTICLE I.

Observations on the Exhausting Machine of Dr. THOMAS STEWART TRAILL, *by Mr.* ROBERT BANCKS, *Mathematical Instrument Maker, in the Strand.*

To Mr. NICHOLSON.

SIR,

IN the present enlightened state of science, it is not to be wondered, that different scientific men should have similar ideas, with the hopes of constructing instruments in the utmost state of perfection. I am led to this reflection by a description of a machine, in your Journal for last month, nearly similar in construction to one made by me ten years ago for Bracey Clark, Esq., a gentleman well known in the scientific world. The same things thought of by different persons.

After some ingenious observations by Dr. Traill, on the impossibility of obtaining a perfect vacuum in the air pump of ordinary construction, he says, p. 63, " it occurred to me, that, if there was a convenient method of using the Torricellian vacuum, it would be preferable to the common air pump, even when best constructed. After various attempts, the annexed figure and description will give an idea Air pump. Torricellian vacuum suggested for it.

of the machine, which I *concieve* well adapted to answer the end proposed."

Plausible in theory.

With all deference to Dr. Traill, I must suppose his ideas are theoretical; and they certainly appear, to those who have not tried such experiment, admirably calculated to produce a perfect vacuum on the Torricellian principle: but I am inclined to think, that the larger an apparatus of this kind is, the more imperfect it must be; since every experienced artist well knows what extreme care is requisite in making barometers of the best construction, where we are obliged to boil the mercury to exclude air and vapour; and as the same means cannot be applied to the instrument alluded to, it follows in consequence, that the air cannot be completely excluded, by the simple manipulation of filling with mercury, and permitting its descent.

The larger the apparatus probably the less perfect the vacuum.

Defect in the cover.

After Dr. Traill has described the instrument, he directs the whole to be filled with mercury, and its cover then to be placed on. Now I believe it is well known from the laws of repulsion, that it will be impossible to fill the receiver in such a manner, as to admit the cover to be put into its proper place, and at the same time exclude all the air necessary for a perfect exhaustion, or *vacuum*. The state of this vacuum too cannot be determined, without a gauge, for which there is no provision in the instrument described.

No gauge.

An apparatus similar to Dr. Traill's.

As I have intruded thus far, I beg to say, that, in the early part of this year, I made a similar apparatus for J. G. Children, Esq. F. R. S. as contrived by himself; and as it appears to have a decided advantage over that of Dr. Traill, I trust a brief description of it may be admitted.

Description of the parts that differ.

It may be sufficient to say, every part is similar in principle to Dr. Traill's, but the recipient of small capacity, and the cover marked B admirably constructed to remove every objection as to the complete filling with mercury. Let fig. A therefore represent the recipient; B the cap, the under side conical as shown; C, a cock with a funnel; D, another cock on the opposite side of the cap. Now after the receiver was filled as high as convenient, the cap (or stopper) was put into the neck. Mercury was then poured into C through the funnel, until it passed out at D (according to the law of fluids finding its level), when both the cocks being turned, communication

communication with the atmosphere was cut off. The cock was then opened at the bottom of the tube E, the mercury descended, and some sort of vacuum was produced.

In order to see the state of this vacuum, as we had an iron tube, I suggested a gauge, which was easily introduced before filling, and floated on the surface of the mercury within the tube. The index of this gauge, passing into the vacuum, exhibited the height of the mercury. But I must confess, though every part was perfectly tight, and I believe well made, the mercury well dried, and the machine much agitated in filling, to dislodge the air, it did not afford the satisfaction sought after. From this failure I am inclined to think, that so good a vacuum cannot be obtained by such a mercurial apparatus, as with a pump of the best construction, that will indicate an exhaustion to the $\frac{1}{10}$ of an inch with the nicest test, a siphon gauge. State of the vacuum tried with a gauge. Not equal to that of a good air pump.

Should you think these observations worthy a place in your Journal, they are at your service.

I am,

Your very humble servant,

Sept. 9, 1808. R. BANCKS.

P. S. Could Dr. Traill's instrument be made perfect, and to supersede the use of our best pumps in nice experiments, many of those that may be deemed of importance must be laid aside, from the expense that would be incurred, as the apparatus could be made only in iron or steel, and would come much more expensive than it does at present. Expensive.

ANNOTATION.

IN giving Dr. Traill's instrument, I never had an idea, that a perfect vacuum would be obtained by it. The great difficulty of freeing the mercury from air, it is probable, must ever prove an insuperable obstacle to our complete success. I think farther it is very questionable, whether the contact between the mercury and the sides of the tube would be so complete, as perfectly to prevent any air from insinuating itself between them. But the grand utility of the machine The vacuum not perfect.

 invented

But it is accomplished at once, and with facility.

invented by Dr. Traill, as well as of those previously invented unknown to him, and here mentioned by Mr. Bancks, appears to me to consist in the being able by its means to exhaust the receiver in a very considerable degree, at a single operation, and without any labour. This great saving of time may be an object of importance on some occasions; as I conceive the exhaustion would be considerable, though not complete, or even equal to that of an air pump of the best kind: for nothing in the remarks of Mr. Bancks contradicts this, and the opinion of Dr. Traill is not merely theoretical, since he rests it on his own experience, though he had not the advantage of an able artist.

II.

On Superacid and Subacid Salts. By WILLIAM HYDE WOLLASTON. *M. D. Sec. R. S**.

Superoxalates have a double portion of acid.

IN the paper which has just been read to the Society†, Dr. Thomson has remarked, that oxalic acid unites to strontian as well as to potash in two different proportions, and that the quantity of acid combined with each of these bases in their superoxalates is just double of that which is saturated by the same quantity of base in their neutral compounds†.

The same law holds in other instances.

As I had observed the same law to prevail in various other instances of superacid and subacid salts, I thought it not unlikely, that this law might obtain generally in such compounds, and it was my design to have pursued the subject with the hope of discovering the cause, to which so regular a relation might be ascribed.

This one case of a more general law observed by Mr. Dalton.

But since the publication of Mr. Dalton's theory of chemical combination, as explained and illustrated by Dr. Thomson‡, the inquiry which I had designed appears to be

* Philos. Trans. for 1807, p. 95.

† See our Journal, p. 19 and 22 of the present vol.

‡ Thomson's Chemistry, 3d Edition, Vol. III, p. 425.

superfluous,

superfluous, as all the facts that I had observed are but particular instances of the more general observation of Mr. Dalton, that in all cases the simple elements of bodies are disposed to unite atom to atom singly, or, if either is in excess, it exceeds by a ratio to be expressed by some simple multiple of the number of its atoms.

However, since those who are desirous of ascertaining the justness of this observation by experiment may be deterred by the difficulties, that we meet with in attempting to determine with precision the constitution of gaseous bodies, for the explanation of which Mr. Dalton's theory was first conceived; and since some persons may imagine, that the results of former experiments on such bodies do not accord sufficiently to authorize the adoption of a new hypothesis, it may be worth while to describe a few experiments, each of which may be performed with the utmost facility, and each of which affords the most direct proof of the proportional redundance or deficiency of acid in the several salts employed. A few easy experiments tend to verify this.

Subcarbonate of Potash.

Exp. 1. Subcarbonate of potash recently prepared, is one instance of an alkali having one half the quantity of acid necessary for its saturation, as may thus be satisfactorily proved. Subcarbonate of potash.

Let two grains of fully saturated and well crystallized carbonate of potash be wrapped in a piece of thin paper, and passed up into an inverted tube filled with mercury, and let the gas be extricated from it by a sufficient quantity of muriatic acid, so that the space it occupies may be marked upon the tube.

Next, let four grains of the same carbonate be exposed for a short time to a red heat; and it will be found to have parted with exactly half its gas; for the gas extricated from it in the same apparatus will be found to occupy exactly the same space, as the quantity before obtained from two grains of fully saturated carbonate.

Subcarbonate of Soda.

Exp. 2. A similar experiment may be made with a saturated Subcarbonate of Soda.

rated carbonate of soda, and with the same result; for this also becomes a true semicarbonate by being exposed for a short time to a red heat.

Supersulphate of Potash.

Supersulphate of potash.

By an experiment equally simple, supersulphate of potash may be shown to contain exactly twice as much acid as is necessary for the mere saturation of the alkali present.

Exp. 3. Let twenty grains of carbonate of potash (which would be more than neutralized by ten grains of sulphuric acid) be mixed with about twenty-five grains of that acid in a covered crucible of platina, or in a glass tube three quarters of an inch diameter, and five or six inches long.

By heating this mixture till it ceases to boil, and begins to appear slightly red hot, a part of the redundant acid will be expelled, and there will remain a determinate quantity forming supersulphate of potash, which when dissolved in water will be very nearly neutralized by an addition of twenty grains more of the same carbonate of potash; but it is generally found very slightly acid, in consequence of the small quantity of sulphuric acid which remains in the vessel in a gaseous state at a red heat.

In the preceding experiments, the acids are made to assume a determinate proportion to their base, by heat which cannot destroy them. In those which follow, the proportion which a destructible acid shall assume cannot be regulated by the same means; but the constitution of its compounds, previously formed, may nevertheless be proved with equal facility.

Superoxalate of Potash.

Superoxalate of potash.

Exp. 4. The common superoxalate of potash is a salt that contains alkali sufficient to saturate exactly half of the acid present. Hence, if two equal quantities of salt of sorrel be taken, and if one of them be exposed to a red heat, the alkali which remains will be found exactly to saturate the redundant acid of the other portion.

In addition to the preceding compounds, selected as distinct examples of binacid salts, I have observed one remarkable instance of a more extended and general prevalence of the

the law under consideration; for when the circumstances are such as to admit the union of a further quantity of oxalic acid with potash, I found a proportion, though different, yet analogous to the former, regularly to occur.

Quadroxalate of Potash.

In attempting to decompose the preceding superoxalate by means of acids, it appeared, that nitric or muriatic acids are capable of taking only half the alkali, and that the salt which crystallizes after solution in either of these acids has accordingly exactly four times as much acid as would saturate the alkali that remains. Quadroxalate of potash.

Exp. 5. For the purpose of proving, that the constitution of this compound has been rightly ascertained, the salt thus formed should be purified by a second crystallization in distilled water; after which the alkali of thirty grains must be obtained by exposure to a red heat, in order to neutralize the redundant acid contained in ten grains of the same salt. The quantity of unburned salt contains alkali for one part out of four of the acid present, and it requires the alkali of three equal quantities of the same salt to saturate the three remaining parts of acid.

The limit to the decomposition of superoxalate of potash by the above acids is analogous to that which occurs when sulphate of potash is decomposed by nitric acid; for in this case also, no quantity of this acid can take more than half the potash, and the remaining salt is converted into a definite supersulphate, similar to that obtained by heat in the third experiment. Sulphate of potash partially decomposed by nitric acid.

It is not improbable, that many other changes in chemistry, supposed to be influenced by a general redundance of some one ingredient, may in fact be limited by a new order of affinities taking place at some definite proportion to be expressed by a simple multiple. And though the strong power of crystallizing in oxalic acid renders the modifications of which its combinations are susceptible more distinct than those of other acids, it seems probable, that a similar play of affinities will arise in solutions, when other acids exceed their base in the same proportion. Many chemical changes influenced by this law.

In order to determine whether oxalic acid is capable of Potas... uniting

unite with a triple quantity of oxalic acid.

uniting to potash in a proportion intermediate between the double and quadruple quantity of acid, I neutralized forty-eight grains of carbonate of potash with thirty grains of oxalic acid, and added sixty grains more of acid, so that I had two parts of potash of twenty-four grains each, and six *equivalent* quantities of oxalic acid of fifteen grains each, in solution, ready to crystallize together, if disposed to unite, in the proportion of three to one; but the first portion of salt that crystallized was the common binoxalate, or salt of sorrel, and a portion selected from the after crystals (which differed very discernibly in their form) was found to contain the quadruple proportion of acid. Hence it is to be presumed, that if these salts could have been perfectly separated, it would have been found, that the two quantities of potash were equally divided, and combined in one instance with two, and in the other with the remaining four of the six *equivalent* quantities of acid taken.

Proportions of acid and alkali.

To account for this want of disposition to unite in the proportion of three to one by Mr. Dalton's theory, I apprehend he might consider the neutral salt as consisting of

	2 particles potash	with	1 acid,
The binoxalate as	1 and 1, or 2	with	2,
The quadroxalate as	1 and 2, or 2	with	4,

in which cases the ratios which I have observed of the acids to each other in these salts would respectively obtain.

Perhaps the law depends on a geometrical ratio of the element.

But an explanation, which admits the supposition of a double share of potash in the neutral salt, is not altogether satisfactory; and I am further inclined to think, that when our views are sufficiently extended, to enable us to reason with precison concerning the proportions of elementary atoms, we shall find the arithmetical relation alone will not be sufficient to explain their mutual action, and that we shall be obliged to acquire a geometrical conception of their relative arrangement in all the three dimensions of solid extension.

Example.

For instance, if we suppose the limit to the approach of particles to be the same in all directions, and hence their virtual extent to be spherical (which is the most simple hypothesis); in this case, when different sorts combine singly there

there is but one mode of union. If they unite in the proportion of two to one, the two particles will naturally arrange themselves at opposite poles of that to which they unite. If there be three, they might be arranged with regularity, at the angles of an equilateral triangle in a great circle surrounding the single spherule; but in this arragement, for want of similar matter at the poles of this circle, the equilibrium would be unstable, and would be liable to be deranged by the slightest force of adjacent combinations; but when the number of one set of particles exceeds in the proportion of four to one, then, on the contrary, a stable equilibrium may again take place, if the four particles are situate at the angles of the four equilateral triangles composing a regular tetrahedron.

But as this geometrical arrangement of the primary elements of matter is altogether conjectural, and must rely for its confirmation or rejection upon future inquiry, I am desirous, that it should not be confounded with the results of the facts and observations related above, which are sufficiently distinct and satisfactory with respect to the existence of the law of simple multiples. It is perhaps too much to hope, that the geometrical arrangement of primary particles will ever be perfectly known; since even admitting that a very small number of these atoms combining together would have a tendency to arrange themselves in the manner I have imagined; yet, until it is ascertained how small a proportion the primary particles themselves bear to the interval between them, it may be supposed, that surrounding combinations, although themselves analogous, might disturb that arrangement, and in this case, the effect of such interference must also be taken into the account, before any theory of chemical combination can be rendered complete.

This merely hypothetical.

III.

Account of Experiments on Sweeping Chimneys, by Mr. GEORGE SMART*.

Premiums for sweeping chimneys.

THE miseries, to which children employed within the flues in cleansing chimnies are liable, induced the Society for the Encouragement of Arts, &c., to offer premiums in the year 1808, to obviate the necessity of so cruel a practice. In the year 1806, the gold medal was adjudged to Mr. George Smart, of the Ordinance Wharf, Westminster-bridge, for the greatest number of chimneys cleansed under his direction by mechanical means, and a particular account of the method used by him for this purpose will be found in the 23d Volume of the Society's Transactions†. During the present session, a gold medal has also been voted to him for the best machine produced to the Society for the intended purpose; an explanatory engraving of which having been given in the volume above-mentioned, renders one unnecessary here‡. The following communication has been received from him, and a complete machine is preserved in the Society's repository for public inspection.

SIR,

Experiments. Rope stiffened with whalebone.

Since the middle of February last, I have been trying experiments in chimney-sweeping; my first was, stiffening a rope with whalebone, but found it would not be portable, and that it would be otherwise inconvenient, as in passing from one room to another in the same house, even with the greatest care, it would be almost impossible to avoid touching the paper on the staircases, particularly where they are narrow. In passing through the street, with such a machine, it would be also very troublesome.

If the brush is made to fill the flue, which ought to be the

* Transac. of the Society of Arts, vol. XXV, p. 97.

† See also our Journal, vol. VI, p. 255.

‡ See Journal, vol. VI, plate 13.

case,

case, a substance of an elastic nature has not power sufficient when the brush is forty or fifty feet up, especially where there are sharp turns in the flues; the force applied to send the brush up is principally spent in friction on the sides of the flues, and of course would soon cut through any flexible substance that combines the whalebone, or other elastic substance used.

My next attempt was to join elastic rods together by screws in the joints, but this plan would not do in passing sharp elbows, as the joints would be strained, and soon unfit for use, and a danger of the joints slipping or breaking, which would leave the brush in the flue.

Elastic rods fastened with screws in the joints.

I then thought of the simple portable machine I have sent for the inspection of the Society; its cheapness, durability, and power of execution, will, I hope, recommend it. I think with perseverance it will abolish the practice of climbing boys; I have used it in several lofty chimnies, and am convinced it may in time become general. I have also sent a rod and curtain that may be fixed to any opening of a chimney piece, from six inches to five feet, without using nails or forks in the common mode, to the injury of the wainscot or chimney piece.

Machine formerly described.

My method of working the machine is, by first putting up the brush, then pressing forward one tube after another, as strung upon the rope, till the brush meets with an elbow in the flue; then it is necessary to tighten the rope by pulling it under the feet, or by means of a small pulley, and putting in one of the small screws to pinch the rope; then make a fresh push, and by shifting the two screws, the one to relieve the other, it will pass the elbow, and possess sufficient stiffness to allow the brush to be forced forward to any height.

Method of using it.

I have tried the heath and hair brushes, and find, that if the flue is well filled, it does not require so hard a substance as heath, as it brings down the mortar with the soot. The brushes of hair, and those formed from the article of which carpet brooms or whisks are made, I think will answer the best for general use.

Brushes.

This

Tried with success.

This is to certify, that Mr. George Smart, of Cambden Town, by means of a machine of his own invention for sweeping chimnies, has made two experiments on my hall and parlour chimnies, to ascertain the practicability of raising the machine through their various windings. The first of these flues measures upwards of fifty feet from the hearth, and the operation was performed with apparent ease, sending down a quantity of soot, together with some wet mortar, although the flue had been recently swept by a regular chimney sweeper. The other from the hearth measures sixty feet, and although there are no less than three elbows in it, running in opposite directions, (as the *boy* informs me), the operation was performed within nine minutes.

JOHN TROTTER.

Soho Square, May 2, 1803.

Certificates were also received from Mr. H. W. Dietrichsen, of Pratt place, Camden Town; Mr. Charles Mill, No. 4, Gloucester place, Camden Town; Mr. John Mason, and the Rev. Jeremiah Joyce, of Camden Town, testifying that they had seen Mr. Smart's machine at work, and that they approved thereof.

SIR,

Much used.

I have the pleasure to inform you, that my machine for sweeping chimnies succeeds far beyond my expectation, and that I am not able to attend to one half of the orders I could have for its use.

His Royal Highness the Prince of Wales has directed, that the chimnies at Carlton-house, also those at the Pavillion, shall for the future be cleansed by my machines. I have also had orders to send to different parts of the kingdom my machines ready made, where they have been the means of providing a comfortable subsistance for poor persons not capable of other business.

Price.

The price of a machine to ascend 60 feet, including rod, curtain, extra brush, and box, is £4 14s. 6d.

Advantages.

There are two particular advantages attending my machines; namely, that of sweeping a great number of narrow flues

flues which no child can get up, and that of extinguishing chimnies when on fire, by placing a wet cloth over the brush, and forcing it up the chimney.

The construction of the machine is so fully shown by the description and engraving of it in page 256, of the 23d volume of the Society's Transactions*, that it will be unnecessary to say more upon the subject.

I am, Sir, your humble servant,

GEORGE SMART.

When I first mentioned this machine in the Journal, vol. VI, I promised to see it tried in my own house. This I have since done, and find no reason to modify any thing there said, as it performed its office to my perfect satisfaction.

IV.

Description of a Machine for Cleansing Chimneys, without the use of climbing Boys. By Mr. JOSEPH DAVIS, *No.* 14, *Crescent, Kingsland Road*†.

SIR,

I Had the pleasure of submitting to your inspection, a model of a machine for the purpose of cleansing chimneys, on the 3d of May, 1803, and which I wished to be brought before the Society of Arts, being convinced, that the approbation of so respectable and enlightened a body of men would greatly tend towards the superseding the use of climbing boys; and I shall, therefore, feel myself greatly obliged if they would examine the machine, and favour me with their opinion on it. Machines for cleansing chimneys.

I am, Sir, very respectfully yours,

JOSEPH DAVIS.

SIR,

The brush part of the model of my machine for cleansing chimnies, which I sent you on the 3d of May 1803, not having

* Or vol. VI of our Journal.

† Transact. of the Society of Arts, vol. XXV, p. 101. The Society voted the silver medal to Mr. Davis, his machine being conceived next in merit to Mr. Smart's. See the preceding Article.

ing any hair in it, I am now enabled to forward you one in a more perfect state, which I had the honour of using in the presence of his Lordship the Bishop of Durham, at the Military Hospital, Westminster, on the 11th of June, 1803, and at the Jennerian Society, in Salisbury square, Fleet street, on the 22d of the same month, and in the same year;—a certificate of which, signed by the gentlemen who were present*, I had the pleasure of sending you. I have also sent three lengths of the rod, (the same as those which were used at the above places), in order to enable the Committee to judge with greater ease of the construction. Permit me nevertheless to observe, that the principle of my rod is so simple and secure, that it may be used in almost every chimney with safety, either by the maid servant, or a boy of twelve years of age; it is calculated both for private and public use, and a stranger to the machine, who used it at the Military Hospital, declared he could sweep any chimney with it, the plan was so good. It will also sweep German and other stove pipes, and flues of almost every description. If the gentlemen of the Society will do me the honour to take the machine into their consideration, I will wait on them by hearing from you, and explain farther particulars.

Used with success.

Its advantages.

I am, Sir, your obedient servant,

JOSEPH DAVIS.

SIR,

Remarks on the machine.

I am glad that I have had an opportunity of giving my opinion of your machine for cleansing chimnies, which I have done by signing the certificate you brought me on Monday last. I am of opinion, that your machine is capable of sweeping a very great proportion of the chimneys in London and elsewhere; perhaps there are not more than one or two in a hundred, which it cannot be raised in. At present, there are (as is well known to all chimney sweepers) some chimneys so small, that boys cannot climb them, so that on the whole I imagine, that your machine will sweep about the same number of chimneys as are swept by boys, though not exactly the very same flues in every instance.

I hope in time we shall convince the public, that they can

* B. M. Forster, Joseph Leaper, and James Hebdin, Esqrs.

have

have their chimneys swept with as much cleanliness, and as effectually with machines, as they have heretofore had them done; and I am convinced that they may be swept as cleanly and effectually as is commonly done with climbing boys, so that the difference to the families who employ your machine will be, that they have the same comfort of a clean chimney, and are satisfied, that they no longer use a method which is full of horrors, and a disgrace to a civilized country.

I remain, Sir,

Your obedient servant,

B. M. FORSTER,

Reference to the Engraving of Mr. Davis's Machine for Cleansing Chimneys, Pl. V, fig. 1, 2, 3, 4.

Fig. 1 Represents the upper part of the machine; A A A A are four brushes for sweeping the four sides of the chimney, they are hinged to the bottom of a tube about three inches diameter; B B show two of the four springs which expand the machine to chimneys of all sizes. The heads of the brushes are made about six inches long, and five wide; and form portions of cylinders, the hair being left longer at the top than the bottom. The hair at the ends of the brushes being left still longer, namely, three inches and a half, for the purpose of sweeping the corners of the chimney. C represents the brush at the top of the machine proper for cleansing the pot; the machine may be used either with or without it, but it is very useful for cleansing stove pipes, by being used alone; it is secured to the top of the rod by means of a spring and socket, as the rods below mentioned. D D D D, four lines to draw the brushes near together by a cord E, so that the machine may be forced up the chimney with greater facility. F, the string to expand the brushes when the machine is at the top of the flue. Description of the machine.

Fig. 2 Shows on a larger scale the top of one of the rods separate. G, the spring attached to it.

Fig. 3 Explains the manner in which the rods are joined together. H being a brass socket fixed at the lower end of each

each rod; the spring G, and upper end of the rod fig. 2, are pressed into this socket so far, that a small point I, on the upper end of the spring, rises through a small hole in the brass socket, and retains the lower end in the socket. Each rod is made of hiccory wood, which being tough and flexible, is particularly well calculated for this use, bending and adapting itself to the different turns it meets with.

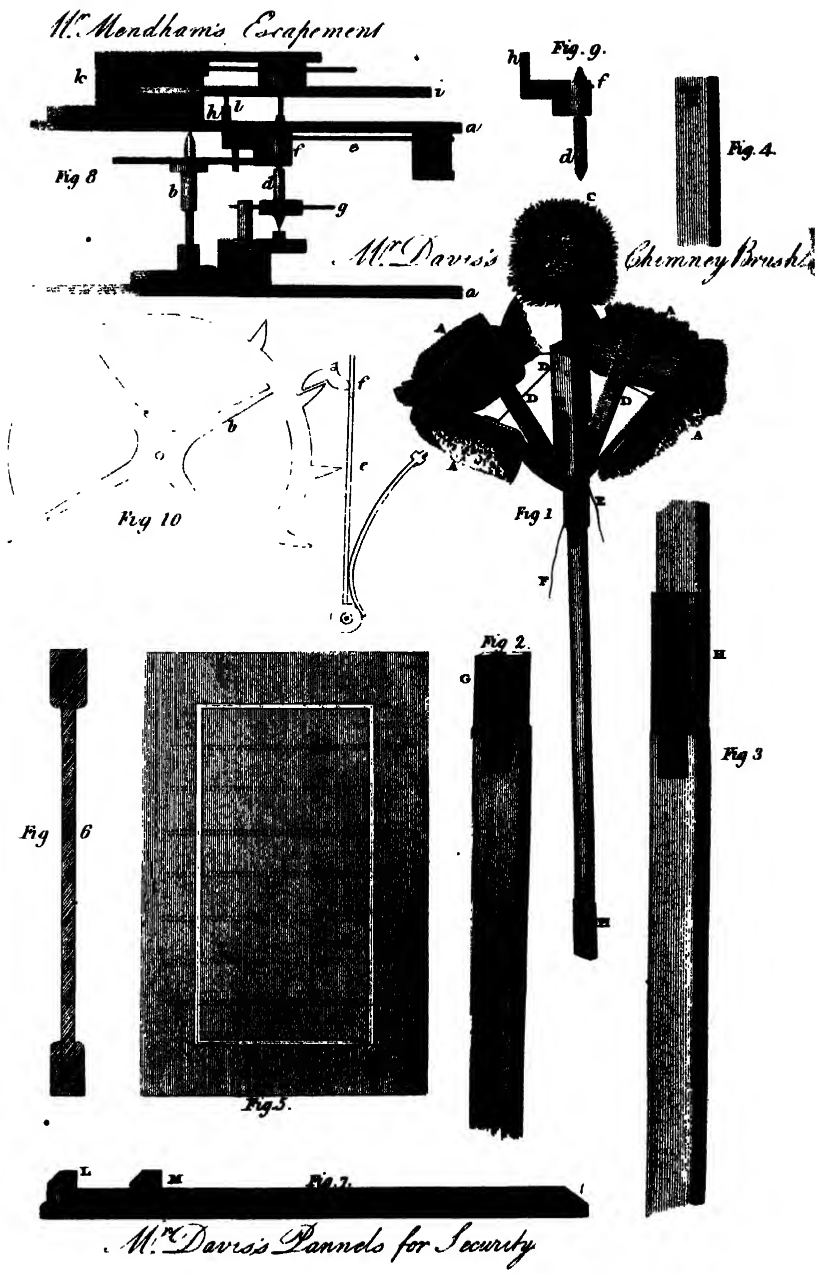
Fig. 4 Shows the key to unlock the rods; it is six inches long, and made from a piece of one of the rods, with a steel stud K in the middle, rising a quarter of an inch, and a brass plate on one side projecting the thickness of the rod to guide it into the socket, that it may be used without looking at the rods.

Method of using it.

The method of cleansing the chimney is, by first entering the brush part of the machine, with the brushes closed, and one of the rods attached to it, up the chimney; the head of a second rod is then slided as above mentioned into the socket of the first rod, and the brush by it forced higher up, a third rod is then slided into the socket of the second, and this mode continued till a sufficient number of rods are added to raise the brush to the top of the chimney. The string E, extending from the brush to the bottom of the chimney, being then pulled, occasions the four brushes to expand to the width of the flue, and to bring down with them in their return all the soot which adhered to the sides of the flue. As the machine is drawn down, the rods are separated one by one by means of the key, and laid upon the hearth till the brush part is brought down, which is then closed and laid with the rods.

The usual precautions must be taken of placing a curtain before the fire-place, to prevent the soot, while falling, from flying about the room.

JOSEPH DAVIS.


Mr. Mendham's Escapement
Fig 8
Fig. 9.
Fig 10
Mr. Davis's Chimney Brush
Fig 1
Fig 2.
Fig 3
Fig. 4.
Fig 5.
Fig 6
Fig. 7.
Mr. Davis's Pannels for Security

V.

Account of an Invention to secure the Pannels of Doors and Window Shutters from being cut out by House-breakers. By Mr. Joseph Davis, *No.* 14, *Crescent, Kingsland Road*.*

Sir,

I HAVE for some time considered, that it would be of great benefit to the public, if a plan could be adopted to prevent the pannels of shutters or doors being cut out by house-breakers; and having tried a great number of experiments, I have at length succeeded in accomplishing the one I have the honour of forwarding to the Society for the Encouragement of Arts, &c.

Desirable to prevent pannels of doors or shutters from being cut out.

This improvement consists in introducing tempered steel wires through the pannels and stiles at the distance of three inches, thereby not only making a door or shutter far superior in strength, but calculated to defy the attempts of the house-breaker in taking out a pannel. This improvement, though so far superior to any hitherto known for appearance and utility, will be attended with less expense. I have submitted the above plan to Messrs. Paynters, Coleman street, likewise to Messrs. Moffat and Co. Paternoster row, and to several gentlemen in that line of business, who all do me the honour to say, that it far surpasses any thing of the kind, that they have ever known or conceived, and that it will completely answer the purpose.

Method of effecting this.

I have also sent the machine on which I bored the pannel, conceiving it to be an additional recommendatoin; with this machine, a boy may with ease bore the shutters, &c. which otherwise might be difficult for a man to accomplish.—I have the pleasure most respectfully to subscribe myself,

Sir,

Your very humble servant,

JOSEPH DAVIS.

* Transactions of the Society of Arts, vol. XXV, p. 101. Ten Guineas were voted to Mr. Davis for this invention.

CERTIFICATE.—We hereby certify, that Mr. Davis's improvement on doors and shutters, to prevent the pannels from being cut out by house-breakers, is the best that we have ever seen; and we are of opinion, that its being known will be an advantage to the public, and do therefore recommend it to the Society for the Encouragement of Arts, Manufactures, and Commerce, for their consideration.

F. PAYNTER and Co. Coleman street.
E. COLEBACH, Minories.
J. TEASDALE, Paternoster row.
W. ROLFE.

April 22, 1807.

Reference to the Engraving of Joseph Davis's Invention for securing Window and Door Pannels, Pl. V, fig. 5, 6, 7.

Explanation of the plate.

Fig. 5. Represents a wooden pannel made in the common way, the dotted lines show the situations of the tempered steel rods within the pannel, the holes through which the rods were introduced on one side being closed up.

Fig. 6. Shows a section of the same, the small dots in the engraving denoting the place of the rods.

Fig. 7. Shows the instrument on which the pannels are laid to be bored. The borer passes through the holes L M of the two upright pieces, which keep the borer in a straight line to act upon the pannel laid upon the frame N.

VI.

Description of a new Watch Escapement. By Mr. S. MENDHAM, *Counter Street, Borough* *.

SIR,

Principle of a new escape.

I Beg leave to lay before the Society a model of a new escapement, the principle of which is, that the balance acts without friction, and the movement in itself very simple;

* Trans. of the Society of Arts, vol. XXV, p. 108. The silver medal of the society was voted to Mr. Mendham for this escapement.

the

the impulse is given without jarring, the inequality of power through the train has no perceptible effect on the balance; and no additional weight, however great, can produce more than a regular and gentle increase of impetus on the balance.

I remain, Sir,

Your most obedient servant,

S. MENDHAM.

SIR,

Having attended the Committee upon Mr. Mendham's escapement, I think it justice due to a man of genius to give my opinion farther upon it. Opinion respecting it.

In viewing mechanical improvements, we should not confine our ideas to their present properties, but should consider what improvements the principle will admit of.

As the principles of Mr. Mendham's escapement, and that of Mr. Mudge's, which obtained a bounty from government, are much the same, I shall compare the one with the other. Compared with Mudge's.

The impulse given to the balance without friction is exactly the same as Mudge's. The remontoir is bent up by the maintaining power in a similar way to that of Mudge's, but from the form of the pallet, which is a plain surface, it is not so perfect. Mudge's, from the form of the pallet, bends the remontoir always to the same place, the other is bent higher or lower according to the force of the maintaining power, but by forming the pallet like Mudge's it would render them alike in this respect. The only other objection is the spring detent, that detains the wheel, when it drops from the pallet of the remontoir; it is the same as that of a detached escapement, consequently exposed to the whole force of the maintaining power. To compensate for these objections, the arc of vibration is not limited like Mudge's, which is of great importance, and, having only one remontoir, it is more simple. It is, therefore, superior to Mudge's in having only one remontoir, and being unlimited in the arc of vibration; it is superior

to the detached escapement in giving the impulse without friction.

I am, Sir,

Your very humble servant,

THOMAS RAMSAY.

Reference to the Engraving of Mr. S. Mendham's Escapement, Pl. V. fig. 8, 9, 10.

Properties of the invention.

In the escapement referred to, there are two principal peculiar properties in the invention, both which I consider superior to any thing of the kind laid before the public; first, the balance is kept in motion without any friction whatever, and in a manner so simple, that even movements of inferior workmanship must go with great accuracy.

Being not in this line of business, or acquainted with any persons in the trade, where I might have had an opportunity of examining different escapements, I certainly labour under many disadvantages; for since I have been honoured with the Society's medal, I have heard of an escapement by which the balance is kept in motion without friction, but being limited in the arc of vibration, complicated, and very expensive in the movement, it renders it much inferior to mine.

The impulse not given by a stroke.

In the second place, the balance is kept in action by an impelling power without any blow whatever; all other escapements, which have fallen within my notice, have kept up the vibration by a direct blow virtually on the balance itself, which I have always considered to be a great disadvantage; for a blow upon any thing of the nature of a spring produces that kind of shock, which can by no means be convenient or serviceable in keeping a steady motion, which is so essentially necessary, but is on the contrary disadvantageous.

Explanation of the plate.

The figure, Plate V, fig. 8, represents the escapement without the rest of the train; *a a* are the two plates of the frame between which the train runs; *b* is the last, or balance wheel of it, with teeth nearly similar to that of the balance wheel of an eight-day clock, moving with the flat face of the tooth forward against the pallet *c* of an upright spindle

spindle *d*; *e* is a locking spring nearly similar to a detached one, having no extra spring to pass to and fro with. Above the pallet *c*, is a very small one, *f*, which is for the purpose of unlocking the wheel, which is better shown in fig. 9; at the lower part of the spindle *d*, is a hair spring *g*, so pinned as to bear the pallet *f* against the locking spring with sufficient power, so that of its own accord it frees the wheel and lays the pin *h* which comes through the plate gently up to the stop, consequently the tooth falls upon the pallet *c*, but so close home to the centre of the spindle, that it has no power to pass it of its own accord; the pin *h* referred to is fixed to the top extremity of the pallet *c*, and rises perpendicularly through the plate *a* some way above the surface.

The balance *i* is fixed on the centre of its spindle, principally on account of equalizing the weight, beside which it is the most convenient to be so; it is supported between the plate *a*, and the cock *k*, precisely over the spindle *d*, consequently the action of each is in the same arch, and the connection is between the pin *h* of the pallet, and the pin *l* of the balance, (a pin fixed in the balance at the same distance from its centre as the pin *h* is from the centre of the spindle *d*, and sufficiently long to touch the pin *h* sideways), there is therefore no friction whatever between them.

Having mentioned the different parts of the escapement, I shall proceed to explain its action. The immediate course of vibration is from the spring *g*. The balance spring is so placed, that the pin *l* of the balance stands near the pin *h* of the pallet. It is to be remembered, that the tooth of the wheel rests on the pallet during the vibration of the balance, so that, when the balance is put in motion, the pin *l* comes in contact with the pin *h*, which stands perpendicularly almost imperceptibly fine, and carries it back; as soon as moved, the tooth of the wheel gives it an extra assistance of about one fifth of a circle, passes and lays the next tooth on the lock; on the return of the balance, the spring *g* applies all its power in urging the balance forward till it comes to the stop, the balance then maintains its motion, and the small pallet *f* having unlocked the wheel, the tooth falls again on the great pallet *c*, and waits the return of the balance.

Mode of its action.

The

The balance carrying the piece *h* back forms a very admirable banking without any extra apparatus, which is generally done by some kind of stop on the hair spring, which must have an irregular effect; the farther the pin is carried back, the stronger the spring operates against it, and from the extent where the piece may be forced back to, there is play for near two whole circles of vibration, without any possibility of upsetting. The balance of the model vibrates about a circle and one third with extraordinary freedom, *though* a course train of four wheels, a large and heavy balance, with only the power of a stout watch spring. I therefore think the power necessary to carry a train with this escapement may be considerably less than any other of a detached nature.

Fig. 9 represents the axis *d* shown separately, in order that the arm and pin *h*, and little pallet *f*, may be seen more distinctly.

Fig. 10 shows the balance wheel *b*, and the method of locking and unlocking.

S. MENDHAM.

VII.

On the Fecula of Potatoes, and some other British Vegetables. By Mr. WILLIAM SKRIMSHIRE, *Jun.*

To Mr. NICHOLSON.

SIR, *Wisbech, Oct.* 11, 1808.

I Take the liberty of sending for insertion in your valuable Journal my promised communication on the fecula of potatoes, and some other vegetables growing in my neighbourhood.

I remain, yours, &c.

WM. SKRIMSHIRE, JUN.

Assertions in a former paper verified by far-

Since the paper published in the ninety-first number of the Journal was written, I have verified by the following experiments

periments two assertions, which were there made, viz. that the quantity of fecula procured from the solanum tuberosum is influenced by the mode of operating, and by the precise state of dryness of the fecula at the time it is weighed. ther experiments.

In August last, 1000 grains of the young roots of the potato called hundred eyes, which had not arrived at their full growth, were grated, and, by the same manipulation as was employed in the former experiments, afforded Hundred eyes potato.

	Grains.
Fine white dry fecula	99
Discoloured fecula	6
Dry pulp	71
Water, soluble mucilage, and extractive matter	817
	1000

The same quantity of these roots being grated and repeatedly triturated in a mortar, with frequent edulcoration, and pressing the pulp with the hands, afforded

Fine white dry fecula	111
Discoloured fecula	20
Dry pulp	44
Water, soluble mucilage, and extractive matter	825
	1000

Thus by greater care and attention bestowed in the last process, we gained twenty-six grains more fecula from one thousand grains of the fresh root, than we procured by the first method of operating. Difference of ·026 of fecula from greater care.

When the fine white fecula procured in the last experiment was at first separated, it was placed in a window facing the south for two days, exposed to the air, and frequent sunshine, until it felt and appeared perfectly dry, it then weighed 127 grains; but being farther dried upon an iron plate, with a gentle heat for two hours, and put into the balance while it was sensibly warm, it weighed 111 grains. 127 grs apparently dry lost 16 by heating,

It therefore lost 16 grains in weight after it appeared to be quite dry. It was afterwards placed in a cellar for twelve hours, and then absorbed 29 from the air of a cellar.

hours, and although it yet appeared dry, it now weighed 190 grains.

Fecula of some potatoes absorb less.

Hence we learn, that dry fecula will absorb more than one sixth of its weight from the atmosphere. But it is more than probable, that this property may vary in the different species of fecula; for some which was obtained from an early potato, subject to a similar experiment at the same time, did not absorb quite one seventh of its weight from the atmosphere.

Fecula still left in the fibrous part.

So far is the common process, that is employed for procuring the fecula of the potato, from separating the whole of this principle from the fibrous part of the root, that when the pulp thus obtained is dried in the air, or by heat artificially applied, an ounce of it boiled for a few minutes in twenty-four ounces of water will gelatinize that quantity, which being sweetened with sugar, and flavoured with a little wine and spice, very much resembles sago that is thus cooked. Indeed it is in my opinion equally palatable and nutritious with that more costly article of food: for which it may be economically substituted in every case, and with every advantage that can be derived from the use of sago.

The pulp should not be thrown away.

For this reason, whenever potato fecula is procured according to the method formerly described, it will be highly improvident and wasteful, to throw the pulp away as refuse, or even to feed pigs with it in its crude state, as has been recommended by some authors; since by being boiled for a very few minutes only, in a large quantity of water, it is converted into the most nutritious food, that any animal can be fed with, and I have no doubt but it will fatten them as effectually, and expeditiously, as any other food that is usually employed for this purpose.

Substitute for coffee.

If the pulp when first separated, and before it is dried, were formed into thin cakes, and roasted with a small quantity of oil or butter, in an iron pan, until it is quite brown and dry, I think it may be used as coffee, and prove an excellent substitute for that costly berry. At least it will prove far superior to that execrable trash, which is often vended under the title of English coffee.

I have frequently formed a very grateful and nutritious beverage from potatoes sliced, roasted to a coffee colour, then

then ground in a mill, and mixed with a sixteenth part of its weight of the best Turkey coffee.

That the preparation of vegetable fecula with boiling water will prove the most economical method of fattening pigs, I have very little doubt. And perhaps it will be found equally useful when employed for fattening the ruminant animals. However, this is an object worth the attention of graziers, and others concerned in feeding animals for the market.

Useful when boiled for fattening pigs, & perhaps other animals.

The mode of preparing the food, which I recommend, is as follows. Let the proper quantity of potatoes be ground, or rather grated in a mill formed for that purpose, in a large quantity of cold water, which should remain at rest for some hours, and then be decanted off. The whole sediment, both pulp and fecula, should be mixed with a proper quantity of water, and boiled in an iron boiler, frequently stirring it before it begins to boil. When it has boiled a few minutes, and is cooled down to a proper temperature to be eaten, it is fit for use.

Method of preparing it.

Should prejudice prove so inveterate as not to trust to potato gruel alone, no one can object to using it as an auxiliary, mixed with barley-meal, boiled potatoes, or pounded linseed cakes.

May be mixed with other food.

The increase of nutriment by boiling farinaceous vegetables in water is so great, that I cannot refrain from recommending its general use; being confident, that the advantage gained by it will amply compensate for the labour and expense attending the operation.

Nutriment increased by boiling.

But it is not as a food for quadrupeds alone, that I wish to facilitate the introduction of potato fecula. I can safely recommend it as a very palatable and nutritious food for man. And however the generality of my countrymen may be disposed to ridicule the notion of being fed upon potato gruel, I fearlessly recommend the following useful and economical preparation of this inestimable root.

Recommended as good for man.

The potatoes being grated, or rasped, in a large quantity of water, and allowed to stand some hours, the water may then be decanted off, and more added, which, after standing an hour or two, may likewise be poured away, and still more added if necessary, until it no longer acquires any taste or colour.

Useful & economical preparation from potatoes.

colour. The whole fecula and pulp are then to be well mixed together, formed into small cakes, and dried in the air, or by a gentle heat. This is a preparation, which will keep for many years—which no family ought to be without, and which is in the power of the poorest family to possess.

Jelly from it, soup, & other preparations.

Half an ounce of this preparation will gelatinize so large a quantity of boiling water, as to afford a sufficient meal for any labouring person in health. It may be sweetened either with molasses or sugar: or being boiled with an onion and pot herbs, and seasoned with pepper and salt, it will make a very palatable, wholesome, and nutritious soup. But should the raw flavour of the potato predominate, as will sometimes happen, when the preparation is newly made, it may be corrected, and the soup improved, by the addition of a little mushroom catsup, allspice, anchovy, or red herring.

Useful for the sick, convalescent,

If this preparation of the potato be boiled with milk, sweetened with sugar, and flavoured with a little wine or spice, it forms the most nourishing and restorative food, that can possibly be administered to the sick and convalescent.

enfeebled, or children.

It is so easily digested, that it soon becomes animalized, even by the impaired functions of a debilitated constitution, Thus it is peculiarly fitted to the digestive organs of the debauchée, and to the languid powers of infancy. And I have known infants wholly nourished for months by this preparation, boiled in milk and water, sweetened with a little sugar.

Excellent remedy for sore throat.

With a larger proportion of the preparation a stiff jelly may be formed, which acidulated with lemon juice, or any other vegetable acid, becomes the best domestic remedy that can be employed, in every species of sore throat.

More elegant preparation.

Those who can afford it may have a much more elegant, though rather a more expensive article, in the pure fecula itself, deprived of all the pulp and fibrous part of the potato. This preparation is so easily made, that I hope to see it introduced into general use. And I do not hesitate to say, it will be found superior in every respect to salep, sago, arrow root, or any of the vegetable preparations of that kind, which have been so pompously advertised, and recommended to the public, by those who are interested in the sale of them.

Potato starch.

Indeed it is already generally known to laundresses under the

the name of *potato-starch*, and they are no strangers to the method of procuring it from the fresh root; but they are not sufficiently aware of the nutritious property which this substance possesses. And it is principally with the view of making it more generally known, that I am induced to lay before the public these experiments and observations*.

The science of nutrition little known.

It will appear ludicrous to many, to assert in the present age of the world, that the science of nutrition is yet in its infancy; but truth obliges us to confess, that such is absolutely the fact. The cause of our ignorance it is not my intention to investigate.

Cookery.

It is, I believe, a general opinion, that the nutriment of our food, especially the vegetable part of it, is greatly increased by cooking. This is therefore an art, which claims the attention of the whole human race. It is an art, so intimately connected with the welfare of our species, that it is absolutely essential to its existence, in a state of civilized society.

Water essential to nutrition.

In the present tottering state of the Lavoisierian doctrine of chemical science, it is fortunately of no consequence to our subject, whether water be a compound substance or a simple element. And we have no fear of contradiction when we assert, that it is essential to the nutrition of animals, as well as of vegetables.

In its simple state assists digestion mechanically

When water in its simple state is taken into the stomach along with our food, its principal effect in assisting digestion is perhaps mechanical only, by giving the food a *certain* degree of consistence, most favourable for the gastric fluid to act upon it, and according to Mr. Home's late important discovery, the superfluous quantity is conveyed into the circulation by the intervention of the spleen*. For as the whole internal surface of the stomach is endowed with the

* Many years ago an article was sold in canisters by the name of sago powder, which I believe was chiefly if not solely made from potatoes: but it fell into disuse, whether from prejudice alone, or from negligence in preparing it, I cannot say. I also remember the fecula of potatoes being strongly recommended as a substitute for salop, particularly as keeping better, as well as for sago, by a writer in the Journal de Médecine. C.

* See Journal, p. 103 of the present vol., and p. 347 of vol. XX.

power

power of producing a secretion, which possesses the property of coagulating albuminous fluids, before the organ is enabled to convert them into chyle, we have some show of reason for supposing a *certain* degree of consistence in the contents of the stomach to be most favourable, if not absolutely necessary, to the healthy action of the digestive organs.

But when chemically combined with fecula becomes animalized.

But when water is gelatinized by its chemical combination with fecula and heat, there is considerable reason to believe, that even the whole of its particles become animalized by the efforts of the stomach. The importance of this idea is such, as to require it to be impressed upon the minds of all, who wish to study the science of nutrition.

Those substances most useful that gelatinize most water.

If therefore the digestive organs have the power of animalizing water, after it has acquired a *certain* degree of consistence, by boiling with farinaceous vegetables, or other substances, we may conclude, that these preparations of vegetables, which in the process of cooking are enabled to consolidate or rather to gelatinize the greatest quantity of water, will be found to afford the largest portion of nutriment, and are consequently the most beneficial to mankind.

Potato fecula recommended.

It is from these considerations, that I am induced to recommend in the strongest terms of approbation the use of potato fecula, as being by far the most economical method of employing this inestimable root.

Common starch not to be taken as food.

From what has been said above, concerning the nutriment of potato starch, I do not wish it to be understood, that the common starch of the shops may be administered as food with impunity. For common starch, after having undergone a slight fermentation, which is sometimes produced by the addition of impure and nauseous ingredients, is still farther contaminated by a metallic oxide, which is probably inimical to the human constitution.

It is for this reason, I wish to retain the name of *fecula*, instead of *starch*, as a generic term for this vegetable principle.

Fecula from various vegetables.

Reflecting upon the nature of this vegetable product, and considering it is equally the produce of seeds and roots, and that, if it be procured with care, it is perhaps equally nutritious from whatever plant it is obtained; whether it be in the form of fecula from the wheat of Great Britain, or in that

of

of Cassava from that deadly poison the Jatropha Manihot of North America; I was induced to suspect, that some other British vegetables, both indigenous and naturalized, might be rendered much more serviceable to our species, than they are at present supposed capable of becoming.

With this view I selected the following.

1. *Æsculus Hippocastanum, or Horse Chesnut.*

Of the fruit of this tree, fresh-gathered, peeled, and skinned, Horse chesnut.

1000 grains, rasped in water with a coarse file, afford

	Grains.
Fine white dry fecula	200
Discoloured or yellowish fecula	32
Dry pulp	80
Water, soluble mucilage, oil, and extractive matter	688
	1000

Thus we find the fruit of the horse chesnut contains more than one fifth of its weight of fecula, the whole of which is converted into animal matter, in the process of digestion! We may therefore, in a time of scarcity, accept with gratitude another rich and wholesome fruit, which has hitherto been held in little estimation.

And indeed at a time when there is no scarcity, those persons, about whose habitations this handsome tree is found to flourish, may profitably employ its fruit in the manner here pointed out.

2. *Quercus Robur, Common Oak.*

The acorn affords a considerable quantity of fecula, but its colour, which is a dirty light brown, similar to powdered salep, will always detract from its value, and prevent its introduction to general use, so long as a more elegant article can be procured with equal facility, and at the same expense. However, I am fully persuaded its colour does not injure its nutritive property. Acorns.

1000

1000 grains of this fruit fresh gathered, and not quite ripe, when peeled and skinned, afford

	Grains
Dry brown fecula	165
Dry pulp	150
Water, soluble mucilage, oil, and extractive matter	685
	1000

3. *Bryonia dioica, Red-berried Bryony, or as it is vulgarly termed, Mandrake.*

Root of Red-berried bryony, or mandrake.

This plant, which is common in this neighbourhood, has a very large, thick, white root, and although it is one of the most violent drastic cathartics, which this kingdom produces, it may, by a similar process to what we have before described, be made to afford a very fine white nutritious fecula, in great abundance.

1000 grains of the fresh root dug up early in May, afford

	Grains.
Dry white fecula	50
Discoloured fecula	45
Dry pulp	50
Water, soluble mucilage, and extractive matter	855
	1000

4. *Arum maculatum, Cuckow-pint, or Wake Robin.*

Root of arum, cuckow pint, or wake robin.

The root of this plant, which is very plentiful in my neighbourhood, although one of the most acrimonious vegetables of British growth, may, by particular management, be converted into a very rich, wholesome, palatable, and productive food.

It is excellent, eaten either boiled or roasted, particularly by the latter mode of cooking. If formed into vermicelli, it is a beautiful preparation. When dried it may be made into bread; and when treated according to the method above mentioned for procuring fecula, a much greater quantity

tity may be obtained, than from any vegetable hitherto operated upon*.

1000 grains of the fresh root, dug up early in May, afford

	Grains.
Very pure white dry fecula	254
Dry pulp	28
Water, soluble mucilage, and extractive matter	718
	1000

Fecula very generally diffused.

The more we reflect upon the general diffusion of this nutritious principle throughout the vegetable kingdom, the greater occasion have we to be seriously and unfeignedly thankful to that Almighty Being, whose extensive benevolence has thus bountifully placed within the reach of man a sufficiency of nutriment, in every corner of the Earth!

VIII.

Remarks on Meïonite, with some Observations on a Paper by Mr. Frederic Mohs, *in which this Substance is considered as a Variety of Feldspar. By Mr.* Tonnelier, *Keeper of the Cabinet of Mineralogy to the Council of Mines*†.

Is meïonite a species, or a variety?

DOES the mineral mentioned by the name of meïonite in the *Tableau méthodique* of Mr. Haüy constitute a distinct species, or is it merely a variety of some species formerly known? Such is the question, that suggested itself to me, on reading a paper by Mr. Frèderic Mohs, lately inserted in

* The root of this plant has been employed for making starch in the island of Portland, from time immemorial. Some years ago the Society of Arts gave a premium to a person of that island for an account of the process, with a specimen of the starch.

† Journal des Mines, vol. XX, p. 165.

in the Ephemerides of baron Moll*, and which I shall here attempt to answer.

First noticed by Romé de l'Isle

For our first knowledge of this substance we are indebted to Romé de l'Isle. This philosopher, guided by the analogy of the figures of their crystals, has united under the name of jacinth, in the second edition of his immortal work on crystallography, several substances, that now form distinct species. These however he was far from considering as the same, though he did not think proper to distinguish them by different names, which he must have invented for the purpose. In the description he has given of the second variety of the jacinth the dioctaedral variety of the meïonite is easily recognized. Beside its locality in the lava of Somma, and the white colour of the mass, which are pointed out, it is there said, that the two quadrangular pyramids of the jacinth, the primitive zircon of Haüy, are separated by a prism of eight unequal faces, alternately hexagons, and rectangular parallelograms; and that the latter of these, produced by the truncation of the edges of the prism, sometimes very narrow and scarcely perceptible, and at other times more or less broad, always answer to the faces of the pyramids; while the hexagonal sides of the prism are always intermediate to these faces: circumstances that agree perfectly with the dioctaedral meïonite. See pl. 6. fig. 5.

as a jacinth with some other substances.

These separated into four species by Haüy,

Mr. Haüy has made four species of the substances described by Romé de l'Isle.

1. The zircon (the jacinth and jargon of former mineralogists), divisible into an octaedron with isosceles triangular faces, which may be subdivided parallel to planes that would pass through the summits and edges of the faces.

2. The harmotôme (cross-stone, crucite, andreolite of the Hartz, staurolite of Kirwan), divisible into a rectangular octaedron, subdivisible on the edges contiguous to the summit.

3. The idiocrase (vesuvian of Werner, jacinth of volcacanoes) divisible parallel to the faces and diagonals of a right prism with square bases, differing little from a cube.

* Ueber Haüy's Mejonit, von Friedrich Mohs: in the Efemeriden der Berg- und Hüttenkunde, herausgegeben von Carl Ehrenbreit Freiherrn von Moll. Band II, Lieferurg I. Nurnberg, 1806.

4. The

4. The meïonite (Romé de l'Isle's white jacinth of Somma), divisible parellel to the faces of a right prism with square bases. Fig. 4.

In characterising these species the learned author of the Theory of the Structure of Crystals has merely applied the general principle, that has served as the base of the classification of the species in the system published by him. On this occasion he had followed the same course, as he had pursued when he separated the heterogeneous substances of which the former mineralogists composed the species they termed schoerl, in order to make a proper distribution of them, or when he demonstrated four distinct species to have been confounded together under the name of zeolite. In short, to constitute the species meïonite Mr. Haüy has employed the means, of which he has so successfully availed himself to effect those useful reforms, for which mineralogy is indebted to him, and the result of which has been a more precise definition of species, with a more regular classification of subjects. The title that meïonite has to be admitted into the system as a species therefore is equally incontestible with those of several other species established by the same gentleman, and generally adopted.

On the principle generally followed by him,

and the results of which have been generally adopted.

Among those who come to study the mineralogical collection of the council of mines, which adds to the means of information derivable from the number and variety of its specimens the advantage of being able to compare the methods of two of our greatest masters, several have put to me the following question: "What species in Werner's system corresponds with that which Haüy has designated by the name of meïonite?" Hitherto I had been unable to answer this question, notwithstanding the pains I had taken to proc re the printed or manuscript syllabuses of the mineralogical lectures delivered at Freyberg. On the one hand I could not suppose that the meïonite, which is at present to be found in all public and private collections, should be wanting in that of Mr. Werner; and on the other hand, in t eries of famil es g ven by that illustrious professor, into wh ch are adopted withou any change of name several species established by the celebrated professor of the Museum of Natural History at Paris, there was no mineral that I

What is the meïonite in Werner's system?

 could

could take for the substance in question. I remained in this uncertainty, till I had read the paper of Mr. Mohs, in which we are informed, that Mr. Werner had not yet adopted the meïonite of Haüy as a distinct species, suspecting it may be nothing more than a simple variety of feldspar. Now the object of this paper is to demonstrate the reality of what is but a simple conjecture on the part of a naturalist, who knows when to doubt, and when to decide. On reading it with that double attention, which the name of Werner and the talents of Mr. Mohs would naturally inspire, I felt in spite of myself the regret of not being able to embrace the same opinion respecting the nature of the mineral that forms the subject of the present paper.

He suspects it to be only a variety of feldspar.

Mr. Mohs admits, that the characters taken from figure exhibit great differences between the meionite and the feldspar; and he confesses, that these differences are little capable of being reconciled: but, as he thinks it not altogether impossible, to reduce the forms of the meionite to a very simple form, which he has observed in the series of forms presented by feldspar, he has flattered himself with being able to justify completely the suspicion of Mr. Werner. The geometrical form in which he gives the proofs alleged in his paper has enabled me to combat them with the same weapons. I appeal therefore to these geometrical reasons, which are employed with the more propriety in the present case, because it is only by the help of the nicest precision, that an able hand has traced the line of demarcation between the meïonite and other species of the mineral kingdom.

They do not agree in figure, but supposed to be reducible to one common form.

It is a principle generally admitted, even by the confession of Mr. Mohs*, that in a mineral species there can be but one primitive form, and one single form for its integrant molecules. To prove therefore, that the meïonite cannot be a variety of feldspar, it is sufficient to demonstrate, that the primitive forms and integrant molecules of these two minerals are very different.

A species can have but one primitive form, and its integrant molecule the same.

* "Es ist ein grundsatz dass in einer gattung nur eine kerngestalt, und nur eine integrirendes molecul, vorkommen koennen; und der orictognost....traegt kein bedenken diesen grundsatz in seiner vollen allgemeinheit gelten zu lassen." Ephemerides of Baron Moll, Vol. II, Part I, p. 15, 1806.

I. Feld-

I. *Feldspar.*

The primitive form of the feldspar, according to Haüy*, is an oblique angled parallelopipedon, in which the angle of incidence between M and P is of 90°, that between M and T of 120°, and that between T and P of 111° 28′ 17″ See in fig. 1 this solid represented in the position given it by Mr. Mohs himself, as being favourable to the comparison he makes of the two substances. Mr. Haüy observes in his treatise, it is true, that the sections parallel to M and P are very clear, and very easy to obtain; while that parallel to T simply shows itself by a changeableness of colour in a strong light. Since the publication of this treatise however, this gentleman has obtained from the feldspar, by mechanical division, nuclei presenting the joint parallel to T in a very clear and decided manner, which he has publicly shown in his late courses of lectures, and some of which he has distributed among his auditors.

Primitive form of feldspar.

The primitive form of the Feldspar once thoroughly ascertained, it remains to be known, whether, setting out from this nucleus, we can obtain by the laws of decrement the forms of the meionite. But the mere inspection of the crystals shows at once the impossibility of this. In fact, the meionite has the four faces of its summit equally inclined to each other and to the lateral faces. Now this symmetry is incompatible with any primitive form but a prism with square bases, as in the mesotype, or a rectangular octaedron, as in the zircon; both which species exhibit forms analogous to those of meionite, but with different incidences. It is altogether the reverse with the forms of the feldspar, which bear in some sort the impression of the irregularity of their primitive form in the want of symmetry of the faces arising on parts similarly situate. The following details appeared to me necessary, to place this proof in a clearer and stronger light.

Can the forms of the meionite be produced by any decrement of this? No.

Fig. 3 represents one of the forms of feldspar, in which the faces M, P, T of figure 1 are preserved, and the face O results from the decrement $\frac{F}{2}$ according to the position the nucleus

Two crystals most favourable to the supposition taken.

* Traité de Minéralogie, tom. 2, p. 591.

nucleus has here. Mr. Mohs has chosen this form among all those of the feldspar, as being the simplest, and best calculated to lead to the object he had in view, the reduction of the forms of the meïonite to those of the feldspar. On the other hand fig. 4 represents the dioctaedral meionite. It now remains, to compare these two forms together; and this, I must apprise the reader, is the essential point of the discussion.

These do not agree.

Mr. Mohs, having measured the angle of incidence between T and P in the crystal of feldspar fig. 3, found, that it agreed manifestly with that between *l* and M in the dioctaedral meïonite, fig. 5. In fact we find by calculation a difference of 21′ only between these angles; the first being 111° 28′, the second 111° 49′. But on proceeding with the comparison, instead of evident resemblances, we have nothing but striking differences. For instance, the angle of incidence between *l* and each of the two sides M, M, is the same; while that between T and M, fig. 3, differs 8° 32′ from that between T and P, since it is of 120°. On the other hand, the angle between O and M is of 116° 21′, and that between O and the face opposite to P is of 124° 15; yet each ought to be of 111° 49′ for the form of the meïonite to agree with that of the feldspar. It is the same with all the other faces, that can arise on the edges or angles of the face T. There are none similarly situate but the faces analogous to M and S, fig. 5, the angles of incidence of which are 90° and 135°. But this is only an accidental resemblance owing to the symmetrical position of the lateral faces in the two nuclei; otherwise we might say, that feldspar is an ore of oxide of tin, since the same angles of incidence are found on the prism of the latter. As to the essential difference between the summits of the crystals of feldspar and those of the crystals of meïonite, this is owing, as has already been observed, to a want of symmetry in the positions of the bases of the nucleus with respect to the lateral faces, which does not allow the faces produced, in consequence of the laws of decrement, to preserve that regularity with respect to each other, which appears in the terminal faces of the dioctaedral meïonite.

The meionite

So far then from acknowledging with Mr. Mohs, that we meet

meet with no crystalline face in the meionite, the inclination of which does not occur among the forms of feldspar, we will venture to request that gentleman, to endeavour to derive the figure of the dioctaedral meïonite, represented fig. 5, from the primitive form of the feldspar, so that the angles of incidence between all the contiguous faces shall agree exactly; I say, exact for in such cases every thing depends on precision; and he will soon convince himself of the impossibility of succeeding. Now this consideration alone is sufficient, to set aside for ever the idea of uniting the meïonite with the feldspar, and decides the question beyond dispute.

not deducible from the primitive form of feldspar.

This decides the question.

The author of the memoir, after having asserted, that all the faces of the meïonite may exist in the feldspar with the same inclinations, finding that the angles of incidence mentioned by Mr. Haüy differ evidently from each other, ascribes this difference to errours in the goniometer, and a want of agreement in the data; and he leaves it to the skilful oryctometer, to remove the difficulty that this want of harmony presents. But the crystallographer finds nothing here to reconcile, since every thing is regular in each of the two crystalline forms. The incidences of the faces have that relation to the primitive forms proper to each species, which calculation, agreeing with observation, indicates in a precise manner by virtue of certain laws of decrement. If the angle of incidence between T and P approach that between l and M; if those between O and M and O and P differ from the latter, as well as from each other; it is because the form of the integrant molecules and the laws of decrement require it. These laws have been determined with the more certainty, as there has been no difficulty in procuring well defined crystals of feldspar and meïonite. They who are fully acquainted with the theory of Haüy, and at the same time know the precision, with which he applies it, see no difficulty in the case. They know, that the angles are rigorously determined by calculations founded on certain laws of decrement, the truth of which is in turn confirmed by the agreement of observation with calculation; and they require no more.

Difference ascribed by Mr. Mohs to errours in measuring.

But the form of each is regular, distinct, and agreeable to the theory.

One example will be sufficient, to give an idea of the accuracy

Instance of the

nicety of Mr. Haüy's measurements.

curacy of the measures given in the work of Mr. Haüy. It is in p. 39 of the first volume of his *Traité de Minéralogie*. Among the number of forms exhibited by sulphuret of iron may be observed the dodecaedron with pentagonal faces. This crystal is divisible parallel to the sides of a cube; which is the form of its nucleus, and at the same time that of its integrant molecules, which perform the functions of subtractive molecules. On each face of the primitive cube two simultaneous decrements are supposed to take place in the additional laminæ; one of two rows in breadth, setting out from two opposite edges; and one of two rows in height, setting out from the other two edges of the same face. The decrements that take place on the faces contiguous to the nucleus follow the same laws, and in directions crossing each other, so that the slower decrement on one face answers to the more rapid decrement on that contiguous to it. The nature of the decrements, added to the direction of the laminæ, gives rise to a new polyedron; the faces of which, becoming level with each other in pairs, are reduced to twelve, instead of twenty-four. The sulphuret of iron has assumed the form of a dodecaedron with pentagonal faces. But it is possible to conceive an infinite number of these dodecaedra, by varying the respective angles of incidence of the contiguous pentagons. What then is the dodecaedron of the sulphuret of iron? is it the regular pentagonal dodecaedron of geometricians? So two learned natural philosophers, Werner and Romé de l'Isle thought: but it is strictly demonstrated by algebra, that such a polyedron cannot result from any law of decrement. The angle of incidence between two contiguous pentagons at a given edge common to both alone determines all the other angles; and it is demonstrated algebraically, that, in the case of decrement of which we are speaking, this angle must be 126° 52′ 8″. Now, on measuring with the goniometer the angle that occurs in the sulphuret of iron, it is found to be nearly 127°: and from this agrement of the calculation with what is actually observed I infer, that the existence of the law of decrement is confirmed. Such is the rigorous method, in which Mr. Haüy constantly proceeds, when he applies his theory to the structure of crystals, to determine species in mineralogy.

Dodecaedral sulphuret of

Not the regular dodecaedron, as supposed by Werner and l'Isle.

II. *Meïonite.*

II. *Meïonite.*

Meïonite.

Mr. Mohs has attempted to raise doubts respecting the primitive form of the meïonite, which he is desirous of assimilating with that of the feldspar: but recent observations made on specimens lately brought from Vesuvius, very well marked and of a good size, have confirmed the angles, both of the primitive and secondary forms, to be the same as given by Mr. Haüy in his Treatise on Mineralogy. This gentleman, having broken crystals of this substance, has perceived joints parallel to the base, the position of which was at first merely conjectured. These joints, it is true, are not so clear as the lateral joints; but this is agreeable to the theory, which, giving a more extensive surface to the bases than to the sides, explains why the sections parallel to the bases are less easy to hit upon than those of the sides, where the points of contact are fewer.

Other characters beside form employed by Haüy.

I have yet compared the meïonite with the feldspar only in respect to form; but there are other characters, such as specific gravity, hardness, lustre, fusibility, &c. The method of Mr. Haüy, which is not purely orictometrical, far from excluding these, calls them in to the assistance of the geometrical characters in determining the species. Now Mr. Mohs says *, if the crystalline forms appear to militate against the union of the meïonite with the feldspar, the other characters taken together will not allow us to part them: otherwise the method ceases to be natural, since it separates what nature has united.

The resemblance in these not so great as asserted.

I shall not stop here to discuss the greater or less resemblance ascribed to the physical or chemical characters of the meïonite with those of feldspar; a resemblance, which does not appear to me so great as is said; for on the one hand the meïonite is strongly scratched by many pieces of feldspar, and on the other, the latter does not melt before the blowpipe like the former with ebullition accompanied by a hissing noise, as has been observed by Mr. Leliévre, member of the council of mines, who is known to be very

* Page 16 of the paper already quoted.

expert

The form alone sufficient to decide the point.

expert in this kind of proof. In the present case, the character borrowed from the form is sufficient. In fact, according to Mr. Mohs, only one primitive form can exist in a species: but the primitive forms of the feldspar and the meïonite are distinguished from each other in all the forms with which we are acquainted: their dimensions have been ascertained by a rigorous theory, the accuracy of which is proved by the agreement of calculation with experience. These alone therefore suffice to distinguish the species: if they did not, they might agree with other species, the forms of which would be different, and then one species would have two different forms, which is contrary to the hypothesis, and implies a contradiction.

Principle of Haüy's method.

Here we see clearly what distinguishes the method of Mr. Haüy. It is founded on the smallest member of characters possible. That which is taken from geometry, which is precise, is always employed, and frequently alone. When the primitive form obtained by mechanical division is a limit, that is to say, a regular, or at least a symmetrical solid, some other character must be added, since it may agree with several species.

The geometrical character not necessary to the student.

However, it is not necessary to determine the molecule of a mineral, in order to find to what species it belongs. This is a labour requisite only to the author of the method, who cannot employ means too precise for the determination of species. He whose object is merely to ascertain the species of a mineral, will find in the method of Mr. Haüy more manageable characters, that will guide him to his end.

Conclusion.

From the details into which I have entered it will be evident to all, who are acquainted with the theory of Mr. Haüy, that the forms of the meïonite are incompatible with those of feldspar, that the integrant molecules of the two differ essentially from each other, and, in fine, that these two substances ought to remain separate in the mineralogical system.

IX.

IX.

Method of finding the Quantity of Refraction from the Distance and Altitude of two known Stars; and of solving by Construction a Problem in Spherical Trigonometry. In a Letter from a Correspondent.

To Mr. NICHOLSON.

SIR,

THE following method of finding the quantity of the refraction by observing the distance and altitudes of two known stars is, as far as I know, new: and as it seems to possess some advantages over the common methods, I will venture to request its insertion in the Philosophical Journal.

New method of finding the quantity of refraction.

Let Z, Pl. VI, fig. 8, be the zenith, S and X the apparent, s and x the true places of the stars.

Let d be the difference between their true and apparent distance; then S s the refraction of the star S $=$ $\frac{d \times \text{tang. } ZS \times \text{cos. } MS \times \text{cos. } MX}{\text{S. } SX \times \text{rad.}^2}$

Demonstration.

Demonstration.

It is evident that $d\ (= Xm + Sn) = \frac{\text{cos. } \angle X \times Xx + \text{cos. } \angle S \times Ss}{\text{rad.}}$ (xm and sn being perpendicular to S X); but $Ss : Xx :: \text{tang. } ZS : \text{tang. } ZX \therefore Xx = \frac{Ss \times \text{tang. } ZX}{\text{tang. } ZS}$; hence by substitution we get $d = Ss \times \frac{\text{cos. } \angle X \times \text{tang. } ZX + \text{cos. } \angle S \times \text{tang. } ZS}{\text{tang. } ZS \times \text{rad.}}$; but $\text{cos. } X = \frac{\text{cotang. } ZX \times \text{tang. } MX}{\text{rad.}}$ and $\text{cos. } S = \frac{\text{cot. } ZS \times \text{tang. } MS}{\text{rad.}}$; hence by substitution, $d = Ss \times \frac{\text{tang. } MX + \text{tang. } MS}{\text{tang. } ZS}$; but sum of tang. : S. of sum :: rad.^{2} : $\square$ of cos. *i. e.* tang. M X + tang. M S : S. S X :: R^2 : cos. M X $\times$ cos. M S; hence $d =$

$$d = \frac{S\,s \times S.\ S\,X \times \text{rad.}^2}{\cos.\ M\,X \times \cos.\ M\,S \times \text{tang.}\ Z\,S} \text{ and } S\,s = \frac{d \times \text{tang.}\ Z\,S \times \cos.\ M\,X \times \cos.\ M\,S}{\text{rad.}^2 \times S.\ S\,X}$$

Q. E. D.

Choice of the stars.

The stsrs should be chosen so as to make the angles S and X acute, as the cos. of an obtuse angle would be negative.

Advantages of the method.

The advantages which this method seems to possess over those which are already in common use, are, 1st, that only one observation is required, as the refraction may vary considerably in the interval between two observations; and 2d, that it does not require the latitude to be known, and that the observation may be taken at sea with the instruments already in common use for lunar observations.

Problem The sides of a spherical triangle given to find its angles.

The following method of solving by construction a problem in spherical trigonometry may possibly be new, and worth your insertion.

Given the sides of the spherical triangle Z S X, to find an angle Z. Let M I and M V (fig. 7) = the secants of the sides Z S and Z X, including the required ∠, take the ∠ I M V = the remaining side; let I Z, Z V = tangents of the sides Z S and Z X, and the ∠ I Z V will be = the required ∠ Z.

Demonstration.

Demonstration.

Let M (fig. 6) be the centre of the sphere, join M Z, M S, M X; draw Z T and Z V tangents to Z S and Z X; hence M T and M V are the secants of those sides, the ∠ T Z V = S Z X, and the ∠ M = the side S X. Hence supposing the triangle T Z V to come into the same plane with T M V, the two triangles will coincide with fig. 7.

Q. E. D.

Extension of the problem.

It is evident, that the cases of two sides and an included angle being given to find the third side, and of two angles and the side included to find the third angle, may be solved by a similar construction.

Yours, &c.

J. B.

Torricellian Vacuum

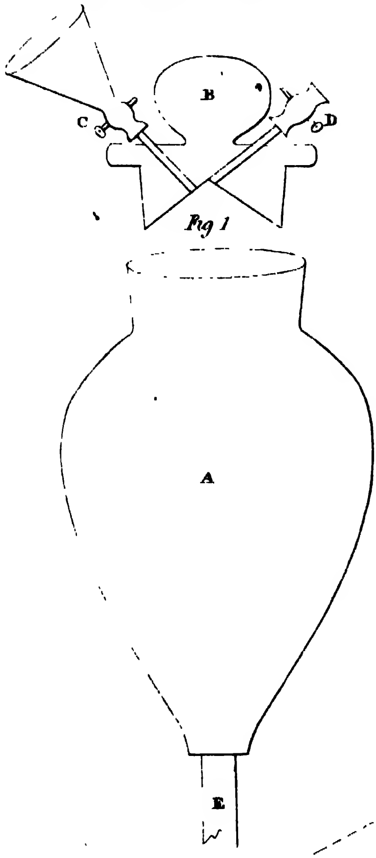


Fig 1

Feldspar

Fig 2

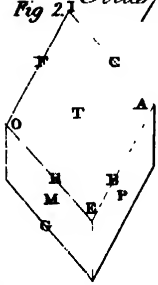
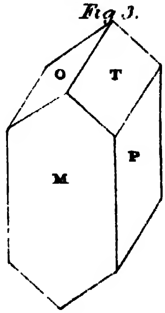


Fig 3.



Meionite

Fig. 4.

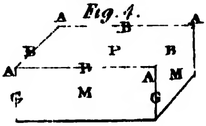


Fig 5

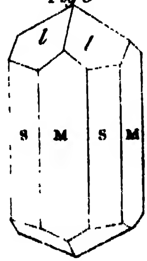
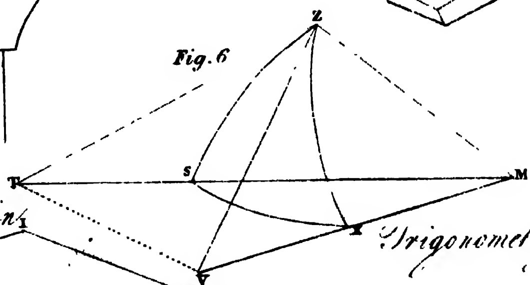


Fig. 6



Problem in Trigonometry

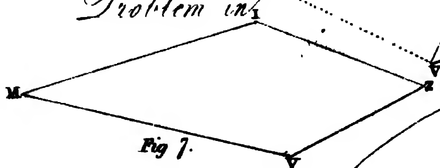
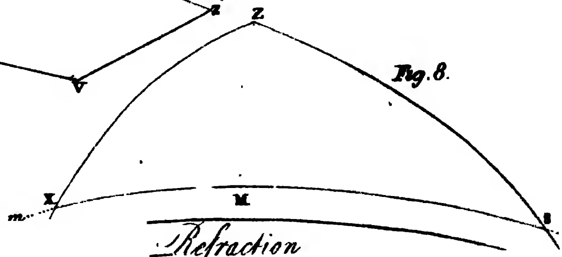


Fig 7.

Fig. 8.



Refraction

X.

Early Account of an Albiness. In a Letter from JOHN BOSTOCK, *M. D.*

To Mr. NICHOLSON.

SIR,

IN examining lately one of the earlier volumes of the Philosophical Transactions, I meet with the following account of an albino, which, as the subject has lately engaged your attention, I have transcribed for insertion in your Journal. It may be thought curious, both as being perhaps the earliest account on record of this peculiar variety of the human species, and also as furnishing another example of its occurrence in a female. It is taken from a paper on monsters, in the 25th volume of the Transactions, for the years 1706, and 1707, bearing the following title. "De monstris, quasi monstris, & monstrosis; item de serpentibus, & Phillippensibus, ex M. S. R. P. Geo. Jos. Camelli. Communicavit D. Jac. Petiver, Pharmacop. Lond. & S. R. S." It is divided into sixty-nine sections, each of which contains a narrative of some uncommon or monstrous production. The account of the albino is placed under the head of "Monstra quæ existebant, A. D. 1700 in insula Catanduen." "Albinam, Hispanis Albinno, vidi Manillæ; erat puella decennis, (proles Morenorum parentum, qui coloris sunt fuliginosi, sed capillitio protenso) albedinis extraordinariæ & insolitæ in admirationem trahentis, & monstruosæ, capilli aureoli, solem ac lucem invitæ ferens. Causam vulgus non phantasiæ sed lunæ influxui tribuit*."

Early account of a female albino.

* "Monsters existing in the year 1700, in the island of Catanduanes.

"I saw at Manilla an albiness, called by the Spaniards an albinno. She was a girl ten years of age, the daughter of negrello parents, who are of a sooty complexion, with very long hair. She had an extraordinary, uncommon, wonderful, and unnatural white skin, golden hair, and was impatient of sunshine or light. The common people ascribe this not to the imagination of the mother, but to the influence of the moon." C.

If

If we except the concluding hypothesis, the account is probably correct; the extraordinary whiteness of the skin, and the great sensibility to light, are well characterized, and precisely resemble what fall under our own observation. It is indeed upon the antecedent probability of the narration, and not upon the credibility of the narrator, that we are to ground our belief; for many of the stories are palpably false and fabulous. The following may be taken as a specimen. A white woman brought forth a child the colour of a negro; the prudent midwife suspecting it to be the effect of some unsatisfied longing of the mother, found upon inquiry, that she had longed for some sardines (a peculiar kind of fish) that she had seen eaten by a black woman. Taking therefore the bones and remains of the fish, she rubbed them over the mouth of the infant, and immediately the dark colour was removed, and a white complexion produced*.

Extraordinary tale.

I am, Sir,

Your obedient Servant,

Clayton-Square, Liverpool, J. BOSTOCK.
OCT. 2d, 1808.

XI.

Remarks on the Doctrines of Chance, in Answer to Opsimath, in a Letter from W. SAINT, *Esq.*

To Mr. NICHOLSON.

SIR, *Woolwich, Sep.* 19, 1808.

I Read in the 91st number of your Philosophical Journal the letter of Opsimath, containing his " scruples as to the truth

* I am inclined to think this account not altogether fabulous, though the assigned cause, and the remedy prescribed, are both palpably absurd. In some instances of difficult labour, the face of the child is so black, lips swelled, and nose flattened, that when born it resembles a young negro; but these appearances soon go off of themselves. Such was probably the case here; and the sagacious midwife applied her remedy time enough, to give it the credit of effecting the removal of what had probably excited astonishmnet and alarm. C.

truth of the elementary doctrines of chance," should you think the following remarks upon the subject likely to remove the doubts of your correspondent, you will, by their insertion, oblige

Your very humble Servant,

W. SAINT.

Sense of the first case of de Moivre.

Opsimath begins by quoting what he deems to be the *sense* of the first case of de Moivre in these words, "Any "one undertaking with a die of six sides, to cast an ace in one "throw, has $\frac{1}{6}$ of the six possible chances in his favour, and the "remaining $\frac{5}{6}$ against him; *the whole six chances being cer-"tainty or at least such in the event of continued trials.*" Now this latter clause of the supposed quotation (I say *supposed* quotation, for Opsimath confesses, that he had not a copy of the work at hand), is not to be found in the first case of de Moivre, or yet in any other case; neither can it be inferred from any thing which he has said on the subject throughout the whole of his work. Indeed had Opsimath proceeded but a few pages farther than the first case, he would have seen, that it was impossible for de Moivre to have considered this as an elementary doctrine of chance, for at Art. 11 he says, "Let a be the number of chances for the happening "of an event, and b the number of chances for its failing, "then the probability of its happening once in any number of "trials will be $\frac{a}{a+b} + \frac{ab}{\overline{a+b}|^2} + \frac{ab^2}{\overline{a+b}|^3}$ &c., till the "number of terms be equal to the number of trials given:" the application of which would give $\frac{31031}{46656}$ for the probability of throwing an ace once in six throws; whereas Opsimath infers, and infers justly, from the expression which he attributes to de Moivre, that $\frac{6}{6}$ or certainty would be the amount of the probability; and this single circumstance, had Opsimath proceeded so far, would have convinced him, that he must either have attributed that to de Moivre which he had never asserted, or else, at least, that he himself must have misunderstood him.

His sense mistaken.

The actual doctrine of de Moivre.

Since the clause abovementioned appears to have been the foundation of Opsimath's scruples, and since this clause

is not to be found in de Moivre, or I may add in any other author that I have seen on the Laws of Chance, perhaps to say any thing farther on the subject may be deemed unnecessary.

Farther statement of doubts.

Lest however the doubts of Opsimath should not be fully removed, let as proceed with him a little farther.—He goes on to say, or rather to quote, that "any one undertaking to "cast an ace in two throws of one die, has for the first proba-"bility $\frac{1}{6}$, as proved: should the first fail, then the second "remains, which is $\frac{1}{6}$ likewise; but the chance of the first "failing is $\frac{5}{6}$, as that of its succeeding is $\frac{1}{6}$; therefore the se-"cond throw has only $\frac{1}{6}$ of $\frac{5}{6}$ for its chance of success, which "added to the chance of casting an ace the first throw, is $\frac{1}{6}+\frac{1}{6}$ "of $\frac{5}{6}=\frac{11}{36}$; the first throw being $\frac{6}{36}$, the second only $\frac{5}{36}$." "This doctrine," Opsimath adds, "I cannot grant"—"because" says he, "nothing can prevent him of the second throw, except his succeeding in the first." Very true, but his succeeding in the first *may* and certainly *will* prevent him of the *second* throw. Opsimath should recollect, that the probability of the event's happening is calculated *before* either throw is made; and that, till the *first throw is made*, it is uncertain whether the *second* will be required; and consequently, that, though the second throw has "the full "force and virtue of $\frac{1}{6}$ chance" *after the first is over*, yet, *before* that event, its value can only be $\frac{1}{6}$ multiplied by the probability that the first throw will *fail*, for on the *failure* of the first depends the necessity of the second—that is, since the probability of the *existence* of the second throw, if I may so term it, is, before the first takes place, only $\frac{5}{6}$; and, *should it exist*, the probability of its producing an ace is only $\frac{1}{6}$; therefore, before either throw is made, the value of the probability of the second is only $\frac{5}{6}$ of $\frac{1}{6}$, that is $\frac{5}{36}$, which, added to $\frac{1}{6}$, the probability for the first throw, gives $\frac{11}{36}$ for the probability on the two.

Answer to these.

Answered by deducing the probability of failure.

Should this consideration of the *dependence* of the second event upon the first fail to remove the scruples of Opsimath, yet, I think there will be no difficulty in convincing him upon the principles, which he has himself admitted, that $\frac{11}{36}$ express the true probability of casting an ace once in two throws. Since the probability of an event's happening, together

gether with that of its failing, makes certainty, which is represented by unity, or 1, therefore the probability, that there will be an ace in one or two throws, together with the probability that there will not be one, is equal to unity. Now the probability, that there will *not* be an ace the first throw is $\frac{5}{6}$, and since, whether there be one or not, the second throw will be equally necessary for determining the probability that there will not be one in *either throw*, therefore this second throw exists with "the full force and virtue of the first, from which no circumstance can deduct," viz. the probability in the second throw will be $\frac{5}{6}$, therefore the probability that there will *not* be an ace in either the first or second throw will be $\frac{5}{6}\times\frac{5}{6}$ $=\frac{25}{36}$, and, deducting this from unity, there will remain $\frac{11}{36}$ for the probability that there *will* be an ace in either the first or second throw, the same as before.

Perhaps there is no branch of the mathematics, which is founded upon fewer first principles than the Laws of Chance; and yet probably there is no subject, the first principles of which are so likely to be misapplied, or misunderstood. One of the principal causes of the errours in our reasoning on this subject, it may safely be affirmed, is the not duly discriminating between events which are dependent and those which are independent; and this seems to have been the source, whence the scruples of Opsimath have originated; scruples, which, it may be asserted, have been entertained by almost every one on his first entrance on this subject. Perhaps, however, what has been said above may tend to remove these doubts, if not, we will conclude by advising Opsimath to acquire correct ideas of the first principles of the Laws of Chance; and if in his inquiries he be guided by right reasoning, we will assure him, that there is no subject in which he will find the conclusions more just, natural, or beautiful.

Laws of chance founded on very few principles, yet very liable to be misunderstood; chiefly from not discriminating between dependent and independent events.

As it often happens, that theory is best understood by practice, and precept best illustrated by example, I have enclosed the following question, taken from the Mathematical Repository, which, as it is rather of a curious nature, you may perhaps deem worthy of insertion; and as the solution, which I have given to it, is founded upon the first and most obvious principles of the Laws of Chance, it may probably be useful

Curious question elucidating the doctrines of chance.

not

not only to Opsimath, but to many other of your correspondents.

Question.

A, with a truths, tells b falsehoods, and B with c truths, d falsehoods, what is the probability of the truth of a circumstance in which they both agree?

Solution.

Probability of the truth of a fact related by two persons.

As they are supposed to *agree* in their relation, they must either *both* speak truth or *both* falsehood. Since then the probability of an event's happening is always expressed by the quotient of the number of times in which it may both happen and fail, it is evident, that in the present case, the probability of a circumstance being true, in which both of them agree, will be expressed by the quotient of the number of times in which they may agree in telling truth divided by the number of times in which they may agree in telling both truth and falsehood. Now the number of times in which they may agree in telling truth will be the number of combinations of a in c, viz. $a\,c$ (for each of the truths a may be told with each of the truths c); and for the same reason the number of times in which both of them may agree in telling falsehood, will be $b\,d$; the true expression therefore for the probability required will be $\frac{a\,c}{a\,c+b\,d}$.

Cor. 1.

Cor. 1. Let a, b, c, and d be all equal, then will $\frac{a\,c}{a\,c+b\,d} = \frac{1}{2}$, now the probability of A's telling truth is expressed by $\frac{a}{a+b}$, which will also $= \frac{1}{2}$; hence the probability of the truth of a circumstance in the relation of which two persons agree who are each in the habit of relating truths as often as falsehood, will be the same as if related by either of them separately.

Cor. 2.

Cor. 2. If a be greater than b and c greater than d, then will $\frac{a\,c}{a\,c+b\,d}$ be greater than either $\frac{a}{a+b}$ or $\frac{c}{d+d}$; for $\frac{a}{a+b} = \frac{a\,c}{a\,c+b\,c}$ and $\frac{c}{c+d} = \frac{a\,c}{a\,c+a\,d}$ Now since a is greater than

than b, and c greater than d, the denominators $a\,c+b\,c$ and $a\,c+a\,d$ to the common numerator $a\,c$ will be each of them greater than $a\,c+b\,d$ and consequently the value of the fractions less than $\frac{a\,c}{a\,c+b\,d}$, viz the probability of a circumstance being true in the relation of which two persons agree, who are each in the habit of relating more truth than falsehoods, will be *greater* than when related by either of them separately.

Cor. 3. If a be less than b and c less than d, then $\frac{a\,c}{a\,c+b\,d}$ Cor. 3.
will be less than either $\frac{a}{a+b}$ or $\frac{c}{c+d}$; viz. the probability of the truth of a circumstance in the relation of which two persons agree, who are each in the habit of relating fewer truths than falsehoods, will be *less* than when related by either of them separately.

Cor. 4. If a be either greater or less than b, and $c=d$, Cor. 4.
then will $\frac{a\,c}{a\,c+b\,d}=\frac{a\,c}{a\,c+b\,c}=\frac{a}{a+b}$; viz. the probability of the truth of a circumstance in the relation of which two persons agree, the one of whom is in the habit of relating an equal number of truths and falsehoods, and the other any number of truths with any other number of falsehoods, will be the same as if related by *that other only*.

Cor. 5. If $d=o$, then $\frac{a\,c}{a\,c+b\,d}=\frac{a\,c}{a\,c}=1$ or certainty, Cor. 5.
viz. the probability of the truth of a circumstance in the relation of which two persons agree, the one of whom uniformly relates truth, and the other any number of truths with any number of falsehoods, will always amount to certainty: as is evident from reflection also, for, in this case, *to agree* they must *both* speak truth.

Cor. 6. Hence also it appears, that the corroborating tes- Cor. 6.
timony of a second person or witness is not *always* an additional evidence in favour of the truth of a circumstance related by the first, for if d be greater than c, $b\,d$ will be greater than $b\,c$, and consequently $\frac{a\,c}{a\,c+b\,c}$ $\left(\text{or its equal } \frac{a}{a+b}\right)$

will be greater than $\frac{a\,c}{a\,c+b\,d}$, viz. the probability of the truth of a circumstance is greater if related by *one person only*, than if related by *two*, when the second is in the habit of relating a greater number of falsehoods than truths.

Cor. 7. Cor. 7. Lastly, if the relations be supposed to be *untold*, or, in other words, if it be supposed that A and B are each *about* to relate a circumstance, the probability that they will both speak truth will be expressed $\frac{a\,c}{\overline{a+b}\times\overline{c+d}}=\frac{a\,c}{ac+ad+bc+bd}$; for the probability of A's speaking truth would be $\frac{a}{a+b}$, of B's $\frac{a}{c+d}$ and therefore of both $\frac{a\,c}{\overline{a+b}\times\overline{c+d}}$.

XII.

Farther Remarks on the Doctrines of Chance. By OPSIMATH.

To Mr. NICHOLSON.

SIR, *October 7th*, 1808.

Not convinced by a correspondent's arguments; I Beg to thank your correspondent Mr. B. H. for his remarks on my letter respecting the Doctrines of Chance, obligingly inserted in your Philosophical Journal for September, although not productive of conviction on my judgment:—they strictly conform with the systems of de Moivre and Thomas Simpson, whose publications are the only works on this subject, which I have seen. But as I fear not to have made the path of reasoning, which leads to my deduction, as plain as it admits, I shall attempt to do so more effectually now, provided my humble essay does not intrude on pages dedicated to the promulgation of so much more valuable information.

and why. The variation of result arises, as Mr. C's remark observes, from six successive throws of one die being assumed equal to 1 simultaneous throw of 6 dice; which position, in my mind, it completely subverts, though supported by the authority of the above celebrated names. Let us compare 2 throws of 1 halfpenny with 1 throw of 2, as to their chances of a head's being

being thrown, which are less complex, and stand on precisely the same base with the throwing of 2 dice.

In the former case I say, I have $\frac{1}{2}$ of success as my first probability; if successful, *I dispense with the second throw, which is however altogether optional on my part, being my privilege by premises.* If unsuccessful in the first, I of course avail myself of the second chance, which, when to be exercised, I cannot estimate in any wise less valuable than its predecessor; and thus I have in all 2 one half chances of success equal to each other, and together equal to assumed certainty on the average of probability: at least such is my conclusion, for I cannot lose without first having had 2 one half chances of winning. Case of a half-penny.

In the latter case I say, I can only lose by throwing 2 tails at once: the probability of throwing one of the halfpence a tail is evidently $\frac{1}{2}$, and of doing so with the other, were this effected, $\frac{1}{2}$ also; therefore the contingency of throwing both tails, is $\frac{1}{2}$ of $\frac{1}{2} = \frac{1}{4}$. Now the probability of failing $\frac{1}{4}$, being deducted from unity, or assumed aggregate of all chances, leaves $\frac{3}{4}$ for the probability of succeeding. Or otherwise, as I can win by throwing 2 heads, for which I have $\frac{1}{4}$ probability, and also by throwing 1 head, for which I have $\frac{1}{2}$ probability, the amount of probabilities to do one of them, is as before $\frac{3}{4}$.

Therefore I estimate 2 throws of one halfpenny, $\frac{1}{4}$ better than 1 throw of 2 halfpence in the chance of throwing a head.

But if it were required to throw 2 heads instead of 1 in the above cases, I estimate the chances of 2 successive throws of one halfpenny, and of 1 simultaneous throw of two halfpence, perfectly alike, viz. each $\frac{1}{4}$; for in this instance, each of the 2 heads supposed to be thrown at once with the 2 halfpence has its value; in the former 1 head is without value at all. And here stands the deceptive point of distinction, the combination of 2 aces with dice, as pointed out by C. If required to throw two heads.

But reasoning even with the disciples of de Moivre, I can not but observe, if they diminish the value of the second throw of 1 die, they ought proportionally to increase the value of the first; for it strictly yields them a twofold advantage, viz. $\frac{1}{6}$ chance of success as admitted, *and likewise* $\frac{5}{6}$ chance of another probability on the failure of that. If the value of one throw be decreased, the other ought to be diminished.

 And

And Mr. B. H. advancing the *entire coincidence of probability* of the 2 dice with one throw, and of the 1 die with 2 throws, as he gives No. 1 throw, $\frac{1}{36}$ advantage over No. 2 throw, he can not in justice withhold from die A, the same $\frac{1}{36}$ advantage over die B, when thrown together; which is exactly the fatal invalidity of its ace, in combination with the ace of A.

I remain, Sir,

Your obliged, and most obedient Servant,

OPSIMATH.

REMARK.

Too many letters sent to be admitted.

NUMEROUS communications of considerable extent, and some of a controversial nature, having been received on the Doctrines of Chance, it was impracticable for the editor to insert them all, notwithstanding the merit of several, as they would have occupied a great deal more room than is consistent with the plan of his work. He has however admitted the letter of Mr. Saint, as containing a curious problem in the application of the doctrine of chances; and has thought it right, that Opsimath should again be allowed to speak for himself.

The word certainty taken in a loose sense.

In answer to the latter gentleman, he would observe, that he appears to be misled by not adhering to the strict meaning of the word certainty, and confounding it with what may properly be termed the right of expectation. In throwing a die, there is no reason we can assign, why a deuce, a trois, or any other of the sides, should turn up preferably to an ace. We have therefore a right to expect, that an ace will be turned up once in six times. Farther, if I do not throw an ace the first time, when I have to throw a second, I have neither more nor less chance of bringing an ace, than I had the first time. Thus, if a stake of thirty guineas were deposited, to which the thrower of an ace would be entitled, I ought to give five guineas for the throw, it being just one fifth of what I should win, and there being one chance for my winning, and five for my losing. If I lost, and chose to throw again, I ought again to give

give five guineas for the throw, as my chance would be precisely the same; and so on for any single throw, however often I might fail. Still, though previous to my having thrown at all I should have a right to expect to throw an ace in six throws, it is not a certainty, for I might very possibly throw some other number every time. In fact, no sum of contingencies, make them as great as we please, can ever amount to a certainty, unless we take all the chances both for and against a thing's happening: And certainty is used with strict precision in the doctrines of chance, as being the sum, not of all the chances of success alone, or of failure alone, but of all the chances both of success and failure. Thus if I had a box capable of throwing ten thousand dice at once, and were to throw them ten thousand times, however great the probability of bringing an ace out of the hundred millions of faces, it would be by no means certain; for ten thousand dice admit of a great variety of combinations, in which no ace appears, and one or other of these combinations might turn up each of the ten thousand times. Now, the beauty of the doctrines of chance consists in this very thing, that they appreciate, not merely what we have a right to expect, in any given instance, but the chance there is of our failing of this expectation. We have a right to expect an ace in six throws of a die. If we throw a greater number of times, we have a right to expect one sixth of the number will produce aces: and the greater the number of times, the nearer the number of aces will be likely to approach to one sixth of the whole; since it is obvious, that there will be the greater chance of more aces than one turning up in some of the series of six successive throws to compensate for those series of six in which none have occurred. Now these probabilities the doctrines of chance, as established by some of the ablest mathematicians, calculate with much precision on solid principles: and it is in this way we find, that, though we have a right to expect to throw one ace in six throws of a die, yet the chance of so doing is $\frac{15625}{46656}$ worse than certainty.

No sum of contingencies amount to certainty.

Doctrines of chance estimate probabilities with precision.

I cannot conclude without observing, that there is considerable merit in a student's refusing implicit reliance on any name, however great; and suspending his judgment till his under-

understanding is convinced; but I trust what has been said will prove sufficient, to remove the doubts of Opsimath. C.

XIII.

Memoir on the Organ by which the fertilizing Fluid is capable of being introduced into the Ovula of Vegetables. By P. TURPIN. *Read at the National Institute, December the 4th*, 1808*.

Discoveries owing to chance, or reasoning & examination.

IN Natural History, as in all other sciences, we are sometimes indebted to chance for discoveries, though they more frequently arise from the deductions of reasoning, and from observation. It is to the last of these I owe the discovery of the organ, which will be described in this paper. This organ, hitherto noticed only in the seeds of the leguminous plants by those celebrated botanists Grew, Gleichen, and Gærtner, and in our own days by Mirbel, according to my researches forms a necessary part of the structure both of monocotyledonous and dicotyledonous seeds.

Coats of a seed.

Before we proceed let us examine what are the principal organs, that the two coats of the ovula exhibit; or, as the readiest way, let us examine the proper coats of a seed arrived at maturity.

Base of a seed contains three different organs.

It is admitted, that the base of the seed, whatever its figure, is always determined by the point which adheres to the placenta. This point, which has received several names, such as umbilicus, hilum, and eye, comprises three distinct organs, each having a different function to fulfil, yet all hitherto confounded by botanists under one term.

The hilum.

The first of these organs, to which the name of hilum is perfectly adapted, is that cicatricula, which is most commonly called the umbilicus of the seed. The lips of this cicatricula, which are sometimes very large, as in the sapota plum, soap-berry, chesnut, and some legumes, inosculate with the exterior vessels of the umbilical cord, which, divid-

* Journal de Physique, vol. LXIII, p. 195.

ing

ing afterward throughout the whole extent of the outer coat, constitute its vascular organization.

The second, which I term omphalodes*, is an aperture placed most commonly in the centre of the hilum, but sometimes toward one of its extremities, and sometimes it is a longitudinal cleft extending from one end of it to the other. This organ, wholly neglected by botanists, forms the passage between two other vascular systems; the first of which, that is the outermost, after having inosculated with the lips of the hilum of the internal membrane, forms its organization in the same manner as that of the outer coat already noticed. In fine, as we observe an omphalodes on the outer integument, we perceive one on the internal membrane, through which the third vascular system passes, consisting of the umbilical vessels, by which the embryo was attached to the parent plant previous to its fecundation, and for some time after†. The omphalodes.

The third is the subject of the present inquiry.

All physiologists are aware, that the point by which the ovula adhere to the ovaries marks the direction in which the radicle will push forth, and this is without exception. For instance in some families of plants, as the dipsaceæ, caprifoliaceæ, and jasmineæ, the ovula are constantly attached to the summit of the cavity of the ovaries, and the radicle is superior: in others, as the campanulaceæ and composite, the point of adhesion is inferior, and the radicle is the same. But the better to generalize our ideas, let us rather say, that the direction of the embryo is always subordinate to that of the Direction of the radicle.

* From the greek ομφαλος, the navel, and οδος, a way.

† Grew appears to be the first, who observed the umbilical vessels of the embryo. These umbilical vessels, the only ones that deserve the name, constitute the innermost vascular system, which, after having passed the coats of the seed by means of the omphalodes, divides into two branches, each of which inosculates with the lobes of the embryo, near the point where they unite with the radicle and plumula It is to be presumed, that these vessels quit the young plant pretty early; for it is extremely difficult, to find any traces of them in ripe seeds, except in those of some of the coniferous plants, the tropæolum, and several of the legumes, in which the two umbilical cicatriculæ are very evident. Umbilical vessels.

seeds

seeds in the pericarp, and that the point by which these are attached always determines the direction of the radicle*.

Point of attachment of the ovulum.

It is known too, that the point of attachment of an ovulum is the umbilicus, with which an infinite number of vessels, destined to form at first the vascular organization of all parts of the seed, and then to convey nourishment to it both before and after fecundation, inosculate in the form of a cord of greater or less length: but how is this fecundation effected? by what way can it reach and penetrate the ovula.

Fecundation.

Common opinion respecting it.

This is certainly an important question to be solved, and on which, to this day, scarcely any thing has been said. The opinion most generally received is, that the prolific vapour descends from the papillæ of the stigma into the placenta, and transmits the fecundation to the embryo through the umbilicus.

Not probable.

But I would here appeal to reason, and ask whether it be conceivable, that the same vessels, and the same aperture on the ovula, can fulfil two such different functions as those of conveying to the embryo nutrition and fecundation, the sources of which are so opposite.

Another organ sought after and discovered.

Such was the reasoning that induced me, to examine carefully whether some other organ beside the nourishing umbilicus did not exist in the ovulum. It was not long before I discovered what I at first suspected: for on the first dissection I observed near the cicatricula of the hilum another aperture, which I could not avoid immediately considering as the organ, by which the intromission of the fertilizing vessels must take place.

This organ always as near as possible to the eye,

This organ, as I have satisfied myself by more than twelve hundred dissections of seeds with one and two cotyledons, is always placed as close as possible to the hilum at the time of fecundation; and if it sometimes recede from it afterward

Direction of the radicle.

* When I say, that the radicle is always directed toward the umbilicus, I mean the umbilicus of the internal membrane. This membrane, to which the direction of the embryo is always subordinate, may sometimes be inverted in the outer integument, as in the lousewort and eyebright: for as there are seeds inverted in the pericarp, for instance in the plum and the hazel nut, so it happens, that the interior membrane is inverted in the outer. This organization requires, that the umbilical cord, after having passed through the exterior omphalodes, should creep between the two coats, to inosculate at the base of the interior membrane, which in this case is opposite to that of the exterior.

terward

terward, it is solely owing to the growth and enlargement of the seed. Its situation near the point of adhesion is such, that the fertilizing vessels may enter it by the shortest way. Thus in the labiati we find it constantly placed toward that part of the hilum, which faces the centre, and is consequently as near as possible to the style. In the liliaceous and leguminous plants, and in general all those that produce capsules in which the seeds adhere laterally, it is superior to the point of adhesion, as we may easily see in the French bean, or any other pulse. I ought also to observe, that it constantly answers to the point of the radicle *, in every seed in which the internal membrane retains the same direction as the outer integument. If on the other hand we consider, that the fertilizing vessels can have no other communication with the embryo but by the papillæ of the stigmata; and if to this be added, that the fecundation is intended solely for the embryo, and influences it alone, which it would be easy to prove by a number of facts; we shall not be surprised, that there are two entrances to the ovula, the first of which, termed by me the micropyle †, serves to give a passage to the fertilizing vessels, while the second, being the umbilicus for conveying nourishment, must be intended for the inosculation of the sapvessels of the parent plant. The sole function of the latter must be that of supplying aliment suited to the delicacy of the young embryo, by furnishing it with juices already in some sort digested and filtered by the extreme tenuity of these vessels.

and opposite the point of the radicle.

Two distinct passages, one for the fertilizing, the other for the nourishing vessels.

The existence of fertilizing vessels has long been proved. They have engaged the attention of physiologists ever since the establishment of the Linnean system; several have traced them from the stigmata to the ovula; and they believed,

Fertilizing vessels.

* Every physiologist knows, that the radicle is the part of the embryo, in which the vital principle appears strongest. This part, which is always the first perceived after fecundation, is likewise that which first lengthens and dilates in germination: accordingly it is toward this, that nature has thought proper to carry directly the fertilizing fluid, placing it opposite the micropyle, through which the vessels intended for this function enter

The radicle.

† Micropyle, from μικρος, small, and πυλη, a gate.

lieved, that these vessels, uniting with the umbilical cord, transmitted the fecundation to the embryo through the umbilicus itself. But as this cord is an assemblage of fertilizing and nutritive vessels, and as there are two apertures at the place where it reaches the ovula, is it not more reasonable to suppose, that it is divided there; that the nutritive vessels inosculate with the umbilicus properly so called; and that the fertilizing vessels pass through the micropyle, to communicate immediately to the embryo the vital principle, or rather that contact, so necessary to the first life of every organic being*?

The micropyle not easily seen in ripe seeds.

The little perceptibility of the micropyle on seeds arrived at maturity is perhaps one of the causes, why it has been overlooked by so many natural philosophers. I said at the beginning, that it had been seen on several of the leguminous seeds by Grew, Gleichen, Gærtner, and Mirbel; but none of these expert observers, except Grew, deemed it of any importauce. Grew ascribed to it two functions, one of which has been already refuted by a number of experiments made on the subject. In the first place he imagined, that this aperture might serve to facilitate the introduction of air and moisture into the seeds at the moment of germination. This notion, which might appear very ingenious and satisfactory when Grew wrote, is inadmissible in the present state of our knowledge. We now know from a thousand experiments, that the stopping up of this aperture, and even that of the omphalodes, with wax or varnish, does not prevent the developement of the embryo. Grew himself, in another part of his work, overturns the use he had at first ascribed to this organ, when he says expressly: "the bean "being enclosed in its skins, it is necessary, that the juices "intended for its nourishment must pass through them by "filtration, and impart to the embryo only the quantity requisite. If the embryo were divested of these, it would "draw too much juice; and as it would be without its fil-

Grew saw it, but mistook its office,

and in another place ascribes a similar office to the coats.

Organized beings have two lives.

* Every organic being has two lives. The first receives its fertilizing principle and nutrition by means of an umbilicus. The second commences at the moment when the embryo or fœtus, having attained its appointed degree of maturity, separates from the placenta, and takes in aliment at a single mouth or at thousands.

"ters,

"ters, which commonly strain the moisture like a very fine "cotton, it would perish, from being unable to feed on too "gross aliment." It is easy to perceive by this passage, that Grew contradicts himself, and that, admitting with more reason the use of the coats, which he very ingeniously compares to filters, he entirely rejects his first opinion of the functions of the micropyle.

This learned anatomist, having observed the micropyle only in a small number of leguminous seeds, in which this organ is constantly placed opposite the point of the radicle, had imagined, that it likewise served to afford this a passage in the process of germination. But how is it to be conceived, that a radicle twenty or thirty times as large as the aperture of the micropyle can issue through it? Besides, where is the person, that has ever had an opportunity of seeing a seed in the state of germination, who has not observed, that the radicle never emerges from its captivity, till the coats, being unable longer to contain the embryo, regularly burst, and thus give a passage first to the radicle, and afterward to the entire young plant? If on the other hand we add to this refutation, that, in a considerable number of seeds, the interior membrane describes a quarter of a circle round itself in the outer integument, as in the commelina and tradescantia, or a semicircle, as in the eyebright, lousewort, and cow-wheat, we shall plainly perceive, that the micropyle of the interior membrane, to which the point of the radicle answers, must be a quarter of a circle distant from the outer micropyle in the former, and half a circle in the latter; and that from this construction it would be impossible for the radicle ever to issue by this aperture, since for this purpose it must wind between the two coats, to come out at last through the external micropyle, which in seeds of this kind is always opposite to the micropyle of the inner membrane, and to the radicle, which is inseparable from the latter.

He likewise supposed it afforded a passage to the radicle.

This improbable from its smallness,

and the flexion it would require in some cases.

If I have been so fortunate as to make known the true way of fecundation in the ovula of vegetables, this is not the only advantage, that vegetable physiology will derive from my labours; for the dissections I have been obliged to make, to generalize the presence of the micropyle in all seeds,

A new law in the science of fruits.

seeds, have enabled me to add a law to carpology, which I conceive to be of such a nature as to admit no exception.

Fruits consist of four distinct parts.

Thoroughly to understand this law, it is necessary to recollect, that all fruits are composed of four very distinct parts, each of which has its own peculiar system of vessels. The first is the pericarp; the second, the outer integument of the seed; the third, the internal membrane; and the fourth, the embryo. But I conceive, that, to facilitate the study of carpology, it will be sufficient to divide fruits into two parts only; the first of these being that envelope of various forms, and of various substance, which botanists term the pericarp; and the second, the seed, which is always united by an umbilical cord to a central receptacle, detached or adherent, or to the inside of the pericarp. These two parts, which have been too frequently confounded together, may be discriminated in future by invariable characters easily distinguished. A seed must always be attached to an umbilical cord, longer or shorter, and always provided with two cicatriculæ at its base, one of which is the nutrimental umbilicus, the other the micropyle: but it cannot in any case have a style, since the styles themselves are nothing more than an elongation of the placenta, or receptacle. Thus the acorn separated from its cup, the chesnut divested of its bristly coat, the nut of the nelumbium taken out of its receptacle, cannot be seeds properly so called, since their coats are terminated by styles. It is undoubtedly for want of knowing this law, that Gærtner, after having described the acorn and chesnut as pericarps, describes the nut of the nelumbium as a simple seed*.

The last three may be considered as one.

Characters by which a seed may be distinguished from a pericarp.

The acorn, chesnut, and nut of the nelumbium, not seeds.

Recapitulation.

On considering what has been said in this paper, it appears, that the micropyle is constantly placed near the umbilicus at the time of fecundation; and that, if it afterward recede from it, this is owing to the dilatation of the seed:

The micropyle distinguishes a seed from an aril.

* The mycropyle may serve likewise to distinguish the seed from the aril. The latter, as Mr. Richard has very justly observed, being only an expansion of the umbilical cord, which covers the seed wholly or in part, cannot have the micropyle, the orifice of which is always in the proper coat of the seed.

that

that in all these seeds, in which the internal membrane preserves the same direction as the outer integument, its situation is always opposite to the point of the radicle: that the umbilical cord, or rather that assemblage of the nutritive vessels belonging to the coats of the seed and the embryo, cannot admit into it the fertilizing vessels: that the extent of these in the plant is and must be only from the papillæ of the stigmata to the embryo: that, after having descended into the placenta, they join the nutritive vessels, and then proceed with them, forming a single cord, to the point where the ovulum is attached: lastly, that at this point there are two apertures, and it appears probable, that the nutritive vessels pass through the umbilicus, and the fertilizing vessels through the micropyle.

Note. When I wrote the above paper, I did not know, that the organ of which I was speaking had already been observed by Geoffroy, though it has not been mentioned by the authors who succeeded him. Geoffroy observed this organ.

Geoffroy's paper is inserted among those of the academy of sciences for the year 1711, and is entitled, Observations on the Structure and Use of the principal Parts of Flowers. His account of it. The author recognises the existence of the micropyle in all seeds, and ascribes to it the same functions as I have done, but with some little difference. I conceive I cannot do better, than describe the passage, in which this gentleman, after having attempted to show, that every grain of the pollen might be a germe, destined to be introduced into the ovulum, and there become a young plant, says, p. 230. "Pursuing this conjecture, it is not difficult to ascertain in what way the germe enters into the vesicles: for, beside that the cavity of the pistil reaches from its extremity to the embryoes of the seeds, these vesicles have likewise a small aperture near the place where they are attached, which is at the extremity of the canal of the pistil; so that the small particle of dust may naturally fall through this little aperture into the cavity of this vesicle, which is the embryo of the seed. This cavity, or kind of cicaticula, is sufficiently evident in most seeds: it may be seen very easily, without the assistance of

"of a microscope in pease, beans, and French beans." Here Geoffroy falls into the same mistake with Grew, when he adds; "The root of the little germe is quite close "to this aperture, and through this same aperture it issues "out, when the seed comes to germinate."

Progress made in vegetable physiology since his time. When we reflect on what Geoffroy says, it is easy to perceive the progress, that has been made toward the knowledge of plants within a century. We can no longer suppose with this naturalist, that the particles of the pollen are germes, as he says; and still less can we think, that these particles can ever be introduced into the ovula by the micropyle. The present state of our knowledge instructs us, that the particles of dust contained in the anthers are so many little bladders filled with a fluid, the only substance to which we allow a fertilizing quality, and the only one capable of being conveyed into the embryoes.

We also know, that the canal found in the centre of the styles of all the monostyle ovaries, and destitute of a central adherent receptacle, cannot in any way promote the process of fecundation, and is nothing but the cavity of the ovary, which is prolonged through the style as far as the stigma.

XIV.

Essay on the Composition of Alcohol and of Sulphuric Ether, By THEODORE DE SAUSSURE. *Read to the Physical and Mathematical Class of the Institute April the 6th.* 1807*.

SECT. I. *Introduction.*

Proportions of the elements of vegetables little known. THE proper methods of arriving at a knowledge of the proportions of the ultimate elements of vegetables are yet so uncertain, and so badly determined, that every inquiry into the subject must furnish useful observations, whatever be the material to which it is applied.

Fermentation. The theory of fermentation can be known only by an analysis of its products, and among these alcohol will always hold an important place.

* Journal de Phissique vol. LXIV., p. 316.

The

The change experienced by this fluid during its transformation into ether has occupied the attention of the ablest chemists. Some have ascribed to ether more oxigen and less carbon than to alcohol*: others have embraced the opposite opinion†. These contradictory conclusions are founded on indirect considerations, and the question must remain undecided, if it be not subjected to a more profound examination. This may be accomplished by two different processes. One consists in analysing the residuum left by the alcohol and sulphuric acid after the separation of the ether: but this residuum, which consists of several different and very compound substances, requires for its examination an immense labour abounding with difficulties. The other process confines itself to the analysis of alcohol and of ether, and to deducing from their difference the changes they have undergone during their transformation. I have chosen the latter mode: as to the advantage of being more easy it adds that of giving us a more absolute knowledge of the composition of these two substances.

Conversion of alcohol into ether.

Contradictory opinions respecting it.

Two ways of coming at the truth.

Analysis of the residuum of the ether too difficult.

Easiest method to analyse both ether and alcohol.

The operation by which I have analysed them consists principally in changing them, by an addition of oxigen, into water, and carbonic acid gas, and estimating from the known composition of these the quantities of carbon, oxigen, and hidrogen, contained in alcohol and in ether.

Mode here pursued.

The proportions of the elements of water and carbonic acid gas have not been ascertained with such precision, as to leave no uncertainty respecting them; and I will not venture to affirm, that those I have adopted, and which I am about to mention, are preferable to any other. It will be easy in this respect to alter the last terms of my analyses, considering, 1st, the volume of the oxigen gas, which I caused to disappear by burning a given weight of alcohol and of ether; and, 2dly, the volume of carbonic acid gas produced at the same time. These two terms alone are the fundamental and important expression of my results. In all the subsequent experiments I admit

Elements of water and carbonic acid not completely ascertained.

1, that 100 parts of water contain 88 parts of oxigen by weight,

Proportions of

* Annales de Chimie, vol. XXIII, p. 43.

† Statique chimique, par Berthollet, vol. II, p. 532.

principles adopted in this paper.

weight, and 12 parts of hidrogen, neglecting the fractions.

2, that two parts by measure of hidrogen gas saturate one of oxigen gas, to form water.

3, that 1000 cubic inches of hidrogen gas, the barometer being at 28 inches, and the thermometer at 10° Reaumur [54·5° F.], at the point of extreme dryness weigh 34·303 grs*.

4, that 100 cubic inches of oxigen gas, under the same circumstances, but at the term of extreme moisture, weigh 512·37 grs.

5, that 1000 cubic inches of carbonic acid gas, under the same circumstances as the last, weigh 693·71 grs.

6, that carbonic acid gas contains its own bulk of oxigen gas.

7, that 100 parts by weight of carbonic acid gas at the point of extreme humidity, contain 26 parts of carbon, neglecting fractions†.

Alcohol at ·792 rectified from muriate of lime.

The alcohol I employed for this analysis was such as Lowitz and Richter designate by the name of *perfect alcohol*, and which they have instructed us how to prepare. Its specific gravity is 0·792 at the temperature of 16° R. [68° F.]. I obtained it by distilling common spirit of wine from half its weight of muriate of lime, dried at a nearly red heat, and drawing off only half the liquor. The product

* The French weights and measures are here retained, as they will be generally throughout this paper. Tr.

100 parts carbonic acid contain 26.14 carbon.

† Since oxigen gas does not sensibly alter its volume when converted into carbonic acid gas, the difference of weight between the two gasses in equal bulks must give the quantity of carbon contained in carbonic acid.

According to my experiments, 100 cubic inches of carbonic acid gas weigh .... 69·371 grs.
100 cubic inches of oxigen gas .... 51·237

Difference............ 18·134

Consequently 69·371 grains of carbonic acid gas contain 18·134 grains of carbon; and by the rule of proportion 69·371 : 18·134 :: 100 : 26·14; so that 100 parts by weight of carbonic acid gas contain 26·14 of carbon.

In this paper I have retained the old Paris measures, to render my results more easily compared with those of others.

was

was still a little aqueous, and was farther rectified by distilling from an equal weight of muriate of lime, and again drawing off only half.

As we cannot expect to attain the truth in a business of so much difficulty as that I had undertaken, but by coming at the same result in different ways, I employed three different processes for decomposing the alcohol. Three processes employed to corroborate each other.

In the first I burned the alchohol by means of a lamp under a receiver filled with a mixture of oxigen gas and common air, as Lavoisier did*, and I examined the products of this combustion. The results obtained by this analysis were the least accurate. First.

In the second I effected the decomposition of the alcohol by the instantaneous detonation of the elastic or gaseous vapour of this liquor with oxigen gas in a Volta's eudiometer. Second.

The third analysis was made by decomposing the alcohol in a redhot tube of porcelain. Third.

Sect. II. *Analysis of alcohol by slow combustion in a close vessel.*

The lamp I employed for burning the alcohol was a graduated glass tube closed at its lower extremity. It was 6 inches high, and 3 lines in diameter internally. The wick was a slender cylinder of amianthus, passing through a metal cap, which kept it in the axis of the tube. I had ascertained by previous observation the weight of alcohol corresponding with each division of the tube, so that I could tell by simple inspection of the column of fluid in the lamp, without taking it out of the receiver to weigh it, the weight of alcohol consumed at the instant of its extinction. Alcohol burned slowly in a close vessel.

I preferred this method to that of Lavoisier, who weighed his lamp before and after the experiment. In this way the lamp could not be taken out of the receiver to weigh it, and to analyse the air in the receiver, till the latter was cold; for it was of essential consequence to note the diminution of the volume of air by the combustion. This cooling requires near an hour; and during this period the high temperature prevailing under the receiver volatilizes a consider- This method preferable to Lavoisier's.

* Journal de Phisique, vol. XXXI, p. 55.

able quantity of alcohol, which in Lavoisier's process was confounded with the liquid that had disappeared from combustion.

Process described.

My lamp, on the wick of which was a particle of phosphorus, was placed with a thermometer under a receiver standing in water*, and half filled with common air. To this I added oxigen gas, and the mixture occupied the space of 651 cubic inches, the barometer standing at 27 inches, and Reaumur's thermometer at 17° [70¼° F]. Before the combustion, according to analysis by Volta's eudiometer, it contained 228·25 inches of oxigen gas, and 422·75 of nitrogen.

Gasses from the combustion of 35·5 grs. of alcohol.

The lamp, kindled by a burning glass, consumed 35½ grs. of alcohol. An hour after it was extinguished, the thermometer under the receiver having fallen to 17° [70¼°], the air contained in it was reduced to 599 cubic inches; and being analysed by limewater and Volta's eudiometer it was found to consist of

Carbonic acid gas	77·87
Oxigen gas	98·42
Nitrogen gas	422·71
	599.

The carbonic acid gas in too small quantities to be absorbed.

I must remark, that the quantity of carbonic acid gas, which formed only 0·13 of the residuum, was too small to be perceptibly absorbed by the water under the receiver at the high temperature at which the process was conducted, and in the short space of time between the combustion and the examination by the eudiometer. I satisfied myself of the truth of this by direct experiment. Besides I found an advantage in substituting water for mercury under the receiver, as a small quantity of alcohol is always volatilised without being burned, even while the combustion is going on. If the receiver be lifted up immediately after the combustion, and while full of vapour, we find this diffuses an alcoholic smell. This vapour, which does not burn because it is in great part aqueous, soon condenses in the water of the trough; but if it stood over mercury, it would increase

Water preferable to mercury because it absorbs the little alcohol evaporated.

* In Lavoisier's experiment the receiver stood over mercury.

the

the bulk of the air in the receiver in proportion to the alcohol it contained, even after cooling.

When the ingenious reasoning of Lavoisier is applied to the results of this experiment, we see, that 35½ grs. of alcohol employed for their combustion 129·83 cubic inches of oxigen gas, and formed 77·87 cubic inches of carbonic acid gas. The liquid residue of the combustion of the alcohol was nearly pure water. Thus the oxigen gas I consumed, deducting the 77·87 cubic inches, that entered into the composition of the carbonic acid, was condensed by the hidrogen of the alcohol in the proportion that forms water, Consequently 129·83—77·87=51·96 cubic inches of oxigen gas must have condensed 103·92 of hidrogen gas, or double their volume. Calculation for the hidrogen.

If the weight of the carbon contained in the carbonic acid gas produced during the combustion be added to the weight of the volume of hidrogen gas just mentioned; we shall find, that the sum of these two elements amounts to little more than half the weight of the alcohol consumed. The weight deficient, or the other products of the analysis, cannot exist in the residual gas, the weight and composition of which are exactly known: it must therefore be in the liquid residue, which I have said is nearly pure water, but which I could not weigh, because it was dispersed in the apparatus. That part of the hidrogen of the alcohol, which did not combine with the oxigen added, combined therefore with the oxigen contained in the liquor itself, to form a quantity of water, which may be estimated by the deficiency in weight. On making the calculation accurately, and reducing the analysis to 100 parts of alcohol, we shall find them to contain Calculation for the oxigen.

Carbon	36·890
Hidrogen	9·365
Oxigen and hidrogen in the proportions that form water	53.745
	100.

Proportion of the elements.

or, by substituting the elements of the water,

Carbon	36·890
Hidrogen	15·814
Oxigen	47·296
	100.

A little azote likewise.

We shall find, that a small quantity of nitrogen must be included in the products of this analysis, for I found ammonia in the water formed by the combustion of alcohol. (See Sect. IV.)

Three experiments nearly agreed.

I repeated this experiment three times with nearly similar results; whence I imagine I made no mistakes, but such as arise from the process itself, which is less accurate than those I shall hereafter describe. I ought however, to compare this analysis with that of Lavoisier by the same process, except in a few minutiæ of detail.

These compared with Lavoisier's.

To reduce our results to expressions, that may be compared with each other, and freed from the different estimations we have followed with respect to the composition of water and carbonic acid gas; I must say, that, in the experiment of Lavoisier, the barometer being at 28 inches, and the thermometer at 10° [54·5° F], 10 grains of alcohol consumed 23·56 cubic inches of oxigen gas, and formed 10·194 cubic inches of carbonic acid gas; while according to mine, 10 grs. of alcohol consumed 34·111 cubic inches of oxigen gas, and formed 20·455 cubic inches of carbonic acid gas, at a similar pressure and temperature.

His alcohol weaker.

Lavoisier has not given the specific gravity of the alcohol he employed. I suppose he must have taken the alcohol considered in his time as the purest, and such as Brisson indicates in his tables, namely at a specific gravity of 0·829. This denotes a mixture of 85·63 parts of perfect alcohol, and 14·37 of water, according to the experiments of Richter, the accuracy of which I have verified. But I find, that, on deducting this proportion of water from Lavoisier's alcohol, and in other respects adopting the results of his experiment, 10 grs. of perfect alcohol would have consumed 27·518 cubic inches of oxigen gas, and formed 11·904 cubic inches of carbonic acid gas. This correction therefore still leaves a great difference between our observations.

Still the difference great.

I ought

I ought to remove one objection, that will no doubt be made against the kind of alcohol I analysed, it having been twice rectified from muriate of lime. Some chemists have asserted, that spirit of wine rectified from this salt acquires properties, by which it approximates to an ether. For this purpose I examined whether spirit of wine rectified by simple distillation, and without addition, would furnish by combustion results similar to those of my former analysis, with the exception of a quantity of water corresponding to that indicated by the difference of specific gravities.

The alcohol not changed by rectifying from muriate of lime.

I rectified common spirit of wine by three successive distillations, without adding muriate of lime, and taking only the first product of each distillation. Thus I reduced it to the specific gravity of 0·8248, at 15° of R. [66° F.]. The process was conducted as in the former experiment. The gas in which the lamp was placed, the barometer at 27 inches, and the thermometer at 15·5° [67° F.], occupied the space of 638 cubic inches, 204 of which were oxigen gas, and 434 nitrogen. By the combustion of 33 grains of spirit of wine this was reduced to 598 cubic inches, consisting of

The experiment repeated with alcohol at ·8248 rectified alone.

Carbonic acid gas	62·79
Oxigen gas	99·12
Nitrogen gas	436·09
	598.

From these results we find, that 100 parts of spirit of wine, of the specific gravity of 0·8248, contain

Carbon	32·24
Hidrogen	8·23
Oxigen and hidrogen, in the proportions that form water	59·53
	100.

Proportions.

Richter's table indicates, that 100 parts of spirit of wine of the density of 0·825 contain 12·8 parts of water. If from these results therefore we would deduce the composition of perfect alcohol, we must substitute 59·53—12·8=46·73 for 59.53 in the preceding analysis. This will reduce the parts repre-

This, allowing for the water in the alcohol, agrees with the former experiment.

representing pure alcohol to 87·2; and, making the calculation for 100 parts, they will contain

Carbon	36·97
Hidrogen	15·87
Oxigen	47·16
	100.

This proves, that the muriate of lime had not affected the alcohol.

The conformity of these results, with those of my first analysis, evidently proves, that spirit of wine rectified without addition is identical, as to its essential principles, with alcohol that has been rectified only twice from muriate of lime. Besides, the latter has none of the characteristics of ether; but retains the properties of alcohol, such as having a weak smell peculiar to spirit of wine, and not in the least ethereal. Perfect alcohol combines with water in all proportions, and its combinations with this liquid undergo changes of density nearly corresponding with those, which common spirit of wine undergoes*. It has a very small degree of expansibility, not at all approaching to that of ether the lowest rectified. Perfect alcohol forms a little soot during its combustion, but only when it is made to burn with a thick and stifled flame. Spirit of wine obtained by simple distillation likewise furnishes some under the same circumstances, but not so much, because it is less concentrated. Ether does or does not form soot according as the atmospheric air has more or less access to it during combustion. The character attempted to be derived from the presence of soot therefore, for distinguishing these two fluids, is not essential.

Possibly it may however, if too much be used, or the rectification too often repeated.

I will not assert however, that alcohol distilled a greater number of times from muriate of lime may not contain a perceptible quantity of ether: for I have observed, after having twice distilled a pound of alcohol from an equal

* I suppose however, that a sufficient quantity of water is first added to the perfect alcohol to reduce it to the density of spirit of wine rectified by simple distillation. Compare the changes of the specific gravity of perfect alcohol by mixture with water in *Die neueren Gegenstande der Chemie* by Richter, with the tables of Blagden and Gilpin in the Philosophical Transactions for 1790 and 1794.

weight

weight of muriate of lime, that this salt, on being dissolved in water, deposited a black substance on the filter, which indicated the decomposition of a small quantity of alcohol; but this black matter was too little to be weighed, and from this result and the preceding we may conclude, that the quantity of alcohol decomposed is so small, as safely to be neglected.

(To be continued in our next.)

XV.

Letter on the Subject of the new Metals. By Mr. A. COMBES.

To Mr. NICHOLSON.

SIR,

IN your Journal for August is a paper by Mr. W. Cooke, of Wolverhampton, in which he states it as his opinion, that the new metal, obtained from potash by professor Davy, is not a simple body, but a compound of hidrogen, electrical fluid, and potash. New metal supposed to be a compound.

If Mr. W. Cooke had taken the trouble to read the elaborate and refined experiments in Mr. Davy's paper (which he might have done, as it has appeared in your Journal) he certainly would never have formed so crude and unwarranted an opinion (which by the by is not original; but has been stated before by Dr. Harrington of Carlisle, in the Gentlemans' Magazine for July, except that the Doctor substitutes the word phlogiston for hidrogen). Mr. W. Cooke would have seen in Mr. Davy's paper, that water is not essential to the production of the inflammable basis of potash; and that, by burning in air, it does not produce a solution of potash, or moist potash, as it ought to do on his supposition, but pure dry solid potash. This an unwarranted opinion, though not new.

Having criticised Mr. W. Cooke's criticism on Mr. Davy, I shall beg the liberty of criticising another communication on the same subject.

In a remark on a letter signed a "Dilletante," you say, (for it seems to come from the editor of the Journal, though from Assertion, that the alkalis have formerly

been supposed to be metallic, said to be false.

from its want of philosophical precision I suspect it has another source) that the alkalis were long ago suspected to be metallic oxides. This is not true. I have read pretty extensively in chemistry, without meeting with such a suspicion. That the alkaline earths and common earths were dephlogisticated metals, has been a very old doctrine; but I remember no such notion with respect to potash and soda. I have looked into Dr. Beddoes's Contributions; but I find no idea there of the alkalis being metallic oxides; but I have met with a much more ingenious suspicion, namely, that metals are compounds of hidrogen and azote, which, since the metallization of ammonia, does not seem so improbable.

I am, Sir, with respect,

Your obedient humble Servant,

A. COMBES.

Chelsea, Sept. 8, 1808.

REMARK.

The author of the preceding letter mistaken.

WHEN a man ventures to affert, that a thing "is not true," because he "has not met with it," he must have considerable confidence in the universality of his reading on the subject, the unremitting attention with which he peruses authors, and the infallible retentiveness of his memory. Admitting however, that Mr. Combes never overlooks a circumstance slightly or incidentally mentioned in a book he reads, and that his memory is too tenacious, ever to let slip what it had once received; it is surely very possible, that he might have wanted opportunity or inclination to read every work, that may have fallen into the hands of a reader much his inferior in talents; and in some of these may have been suggested hints, that have hitched in a memory far less tenacious than his. To speak with "philosophical precision" indeed, he should merely have said, that he did not recollect ever to have met with such an opinion. I can only say,

as the opinion s mentioned by Fourcroy,

that, in a book so commonly read as Fourcroy's Chemistry, the opinion, that both potash and soda are of a metallic nature is mentioned, if not directly, by implication. His words

words are, in my translation of the last edition, vol. II, p. 272, art. barytes*, "the opinion relative to the pretended metallic nature of barytes, as well as of *the other salifiable*, and particularly earthy bases, will be nothing but a mere hypothesis." Now as the term *salifiable bases* is used by Fourcroy to signify the earths and alkalis; and as it cannot by any means in this passage be confined to the earths, since he immediately particularizes these, as if the opinion of their metallic nature had been more prevalent, which is undoubtedly the fact; he clearly alludes to the opinion, that potash, soda, and even ammonia were of a metallic nature. The very slight way in which he records this opinion is owing to his considering it highly improbable.

But the same opinion is given more decidedly and directly by a writer of our own country, Mr. Robert Kerr.

And is advanced by the translator of Lavoisier.

In his translation of Lavoisier's Elements of Chemistry, 2d edition, Edinburgh, 1793, p. 217, the following passage occurs in the text. "We are probably only acquainted as "yet with a part of the metallic substances existing in na- "ture, as all those which have a stronger affinity to oxigen "than carbon possesses are incapable hitherto of being re- "duced to the metallic state, and consequently being only "presented to our observation under the form of oxides, are "confounded with earths. It is extremely probable, that "barytes, which we have just now arranged with earths, "is in this situation; for in many experiments it exhibits "properties nearly approaching to those of metallic bodies. "It is even possible, that all the substances we call earths "may be only metallic oxides, irreducibleby any hither to "known process."

And the translator adds, p. 219, an entirely new section, sect. 6. *On the metallic nature of the earths*, in which he relates the experiments of Ruprecht and Tondi, taken from "Baron Born's description of the Cabinet of Mademoiselle "Raab;" who, as is well known, obtained metallic masses by treating barytes, magnesia, and lime severally with carbonic matter in a strong heat. This history need not be here again revived, but it is material to add, that the luminous speculations of the translator, who expressly, p. 214, mentions

* Original, vol. II. p. 196.

mentions the alkalis as being probably metallic substances, and those of baron Born, appear to include in a general way all that the researches of Davy have realized by the skilful management of an agent, the chemical power and habitudes of which were discovered and extensively applied in this country within a few weeks after the knowledge of it was transmitted to us by Volta, one of the patriarchs of electrical knowledge and invention. It is no derogation to the merits of Davy, that he has explored the processes of nature by simplicity of investigation, and clear deductions grounded upon a knowledge of the anticedent analogies, to which he has put in no claim, and upon which it is probable he may not at present set any high value.

XVI.

Remarks on Ignition by compressed Air. In a Letter from J. A. De Luc, *Esq.*

To Mr. NICHOLSON.

SIR, *Windsor, 15th Oct.* 1808.

Ignition by compressed air.

I HAVE found in your No. 89, the following article: "Question respecting the *ignition* of Tinder by *compressed* "*Air.*" In this question, as well as in the reply, the *ignition* is supposed an effect of the *compression* of the *air* itself; and this is the object on which I take the liberty of addressing to you some remarks.

The air not much condensed, for the piston does not recoil.

That this effect is not produced by the *compression* of *air*, is proved by some circumstances of the operation; for in fact, the *air* does not arrive to a great *density* in the instrument. If the original quantity of *air* remained sensibly in the barrel; when the piston is let free, it would recoil as much as it has been forced in, which is far from being the case. A great part therefore of that *air*, is forced out in the operation; and this even is necessary to the effect, for, if the piston did not reach almost the bottom of the syringe, the *ignition* of the tinder would not take place; and such a motion would be impossible, did all the *air*, or its greatest part, remain in the barrel.

It

It is not therefore, the *condensation* of *air*, which produces the *ignition*; it is the *condensation* of the immediate *cause* of *heat*; sometimes called *matter of heat*, but which, in all the records of Natural Philosophy, is named *fire*, *igneous fluid*, or their correspondents in all languages ancient and modern; and it has always been considered as an *expansible fluid*, of great power of *expansion*, when arrived to a great *density*. The cause is the condensation of calorie:

This is the cause of our phenomenon; it is produced by the same kind of operation, which brings to a *red-heat* a slip of iron very rapidly *hammered*; and that cause is the *condensation* of *fire*. That *fluid* may be *compressed* or *rarefied* in the same manner as *air*, by mechanical means. Thus in the *air pump*, which furnishes both examples at once; at the same time that the *manometer* rises or falls, by *condensing* or *rarefying* the *air* in the receiver, the *thermometer* rises or falls in it, by the *condensation* or *rarefaction* of the *free fire* mixed with the air; and both effects are produced by lessening or enlarging the *space* in which fixed quantities of the respective *fluids* are contained. as when iron is hammered red-hot. Similar phenomenon in the air pump.

The only difference between the two cases proceeds from that of the permeability of bodies to these fluids. The vessels being impermeable to *air*, and made *air-tight*, the condensation or rarefaction of *air* may be produced as *slowly* as convenient, without changing the effects: whereas no vessel being *fire-tight*, the operation requires a great *rapidity*. If the same *number* of strokes of a hammer, which, by *rapidly* succeeding each other, bring a slip of iron to *incandescence*, were struck at great intervals; or if the piston which, being *rapidly* moved up to the bottom of the syringe here in view, produces the *ignition* of the tinder, is moved *slowly*; these effects are not produced: because the condensed *fire* has time to escape through the pores, in the first case of the iron, and in the latter of the barrel. A difference in the cases from fire permeating all bodies, whence rapidity necessary

This, Sir, is what appears to me the cause of the ignition of tinder in that apparatus, which I beg you will consign in your very useful repository, if you think proper.

I am, Sir,

Your most obedient humble servant,

DE LUC.

XVII.

XVII.

On the Disadvantage of Jewelled Holes in Clockwork. In a Letter from Mr. W. Walker, *to Mr.* J. Barraud.

Dear Sir,

State of the jewelled holes of a transit clock.

I AM sorry to have delayed so long the account you wished of the state in which I twice found the jewelled holes of my transit clock, when I took it to pieces: as the vibration had each time fallen off, from being on each side the perpendicular 2° 10′ 10″, and were then no more than 1° 30′ 40″.

After 15 months going, and 2 months rest, would not go.

In July 1805, under your direction, the clock was cleaned, and was kept regularly going till Oct. 1806, when I went from home for two months. On my return on Dec. 6th, I wound it up, but could not make it go even when I added about two pounds weight more to the clock weight. I therefore took it to pieces, and found the oil very fluid in all the holes, except those which were jewelled, where it was almost black, and very glutinous. It required great force, and some dexterity, to draw out the spindle that carries the seconds hand. I set the clock going again on the 7th of Dec., and it immediately threw out its full vibration on each side = 2° 10′ 10″; and continued to go with its usual excellence, till towards the end of Oct. 1807, when it again fell off considerably; and gained very much on its general rate. Therefore, on Nov. 23d, 1807, I again took it to pieces; found all the jewelled holes extremely foul, black, and clogged; and separated the jewels, which were strongly adhesive: yet the oil on the pallets was very fluid, and in a good state in all the brass holes. Before this cleaning the clock had gradually thrown out less and less for two months preceding, and was at this time no more than 1° 30′ 40″ on each side, but on fresh oil being applied, it immediately became = 2° 10′ 10″ on each side; and has gone with such excellence ever since, that I cannot forbear transcribing the latter part of my Journal;

Oil fluid except in the jewelled holes.

Set a going again.

In 10 months again its going affected, from the foulness at the jewelling.

Excellence of its going.

nal; although in many other places, where the observations have been carefully made, I might have selected you a longer period; but the variety of this month in temperature, the thermometer in the clockcase having been at 16° and at 47°, is perhaps as severe a test as could be brought forward.

Rate of my transit clock made by Mr. Barraud.

1807.			
From Nov. 26			"
To Dec.	9	....	+ 1,3
	10	....	+ 1,4
	14	....	+ 1,3
1808.			
Jan.	3	....	+ 1,3
	4	....	+ 1,1
	6	....	+ 1,2
	12	....	+ 1,2

These were the only days on which I could get an observation.

I remain, dear Sir,

Your obliged Friend,

And humble Servant,

W. WALKER.

Manor House, Hayes, Middlesex,

20th JANUARY, 1808.

SCIENTIFIC NEWS.

Wernerian Natural History Society.

AT the last meeting of the Wernerian Natural History Society, (1st August) Dr. James Ogilby of Dublin read a very interesting account of the Mineralogy of East Lothian, which appeared to have been drawn up from a series of observations made with great skill, and was illustrated by a suite

Mineralogy of East Lothian.

suite of 350 specimens laid upon the table.—As the county is in general deeply covered with soil, and profusely clothed with vegetables, the determination of the different formations must have been a work of considerable labour; and the skill, judgment, and perseverance of the observer, must have been frequently put to the trial. The doctor, after describing the physiognomy or external aspect of the county, gave a particular account of the different formations of which it is composed. They are as follows:—transition, independent coal, newest floetztrap, and alluvial. When describing the different transition rocks, he alluded particularly to the supposed granite of Fassnett, (described by Professor Playfair in his Illustrations of the Huttonian Theory*), which he proved to be a stratified bed of transition greenstone. The description of the rocks of the newest floetz-trap formation was particularly interesting, not only on account of the beautiful transitions he pointed out, but also as it proved the existence of a considerable tract of these rocks in Scotland, where their occurrence had been disputed. He enumerated and described the following members of this formation:—traptuff, amygdaloid, clay-stone, basalt, porphyry slate, and porphyry slate inclining to greenstone. He found the traptuff, which is a coarse mechanical deposit, forming the lowest member of the series, and resting immediately on the coal formation: on this tuff rests amygdaloid containing fragments: above this amygdaloid is common amygdaloid free of fragments; this, in its turn, is covered with basalt: the basalt gradually passes into and is covered with porphyry slate: and the porphyry slate, in some instances, appears to pass into greenstone, which forms the uppermost portion of the formation:—so that we have thus a beautiful series of transitions from the coarse mechanical, to the fine chemical; that is, from traptuff to porphyry slate inclining to greenstone. The doctor also remarked, that the amygdaloid contains crystals of feldspar which have an earthy aspect; the basalt, crystals of feldspar possessing the characters of common feldspar; and the porphyry slate, glassy feldspar;—facts which coincide with, and are illustrative of the increasing fineness of the solution, from the

Newest floetz-trap formation.

* Page 328.

oldest

oldest to the newest members of the formation. In the course of his paper, the doctor gave distinct and satisfactory answers to the following queries, which had been proposed by Professor Jameson: 1. Does the Bass Rock in the Frith of Forth belong to the newest floetztrap formation? 2. Does the sienitic greenstone of Fassnett in East Lothian belong to the transition rocks, or to the newest floetztrap formation? Are the geognostic relations of the porphyry slate, or clinkstone porphyry, of East Lothian, the same as in other countries? The doctor announced his intention of reading, at the next meeting of the Society, a description of the different veins that occur in East Lothian, and of giving a short statement of the geognostical and economical inferences to be deduced from the appearances which he has investigated with so much care. It is indeed only by investigations like those of Dr. Ogilby, that we obtain any certainty respecting the mineral treasures of a country; and such alone can afford us data for a legitimate theory of the formation of the globe.

New species of fasciola occasioning a disease in poultry.

At the same meeting, a communication from Col. Montague was read, describing a new species of fasciola, of a red colour, and about an inch long, which sometimes lodges in the trachea of chickens, and which the colonel found to be the occasion of the distemper called the *gapes*, so fatal to these useful tenants of the poultry yard. The knowledge of the true cause of this malady will, it is hoped, soon be followed by the discovery of a specific cure: in the mean time, a very simple popular remedy is employed in Devonshire: the meat of the chicks (barley or oat meal) is merely mixed up with urine, in place of water, and this prescription is very generally attended with the best effects.

To CORRESPONDENTS.

Mr. Gough's answer to Mr. Barlow, and the communication from Mr. Moore, in our next.

O's letters will be attended to.

Meteor-

METEOROLOGICAL JOURNAL

For OCTOBER 1808,

Kept by ROBERT BANCKS, Mathematical Instrument Maker, in the STRAND, LONDON.

SEPT. Day of	THERMOMETER. 9 A. M.	9 P. M.	Highest.	Lowest.	BAROMETER, 9 A. M.	WEATHER. Night.	Day.
26	54	60	61	48	30·09	Cloudy	Fair
27	50	47	56	42	30·01	Fair	Ditto
28	44	43	50	40	29·68	Cloudy	Ditto
29	43	43	54	38	29·44	Fair	Rain
30	42	45	55	40	29 58	Ditto	Ditto
OCT.							
1	42	44	52	39	29·90	Ditto	Fair
2	45	52	54	45	29·76	Cloudy	Rain
3	50	49	54	46	30·02	Fair	Ditto
4	51	52	55	47	30·25	Ditto	Ditto
5	52	52	60	46	30·18	Ditto	Fair
6	51	56	58	51	30·10	Ditto	Ditto
7	54	52	54	45	30·09	Cloudy*	Ditto
8	48	46	51	40	29·34	Rain	Rain
9	46	42	53	42	29·78	Fair	Fair
10	52	49	55	42	29·78	Ditto	Ditto
11	46	53	56	44	30·04	Rain	Rain
12	48	50	52	37	29·97	Cloudy	Ditto
13	40	44	53	41	30·10	Ditto	Fair
14	47	45	52	40	29·50	Ditto	Rain
15	43	46	48	41	29·21	Fair	Ditto
16	44	44	49	40	29·60	Ditto	Ditto
17	45	41	48	40	29·59	Ditto	Fair
18	42	47	50	39	29·75	Rain	Ditto
19	42	46	50	42	29·52	Fair	Rain
20	42	46	51	42	29·70	Cloudy	Ditto
21	47	42	51	41	29·34	Fair	Ditto
22	44	41	49	35	29·49	Ditto	Fair
23	40	48	50	47	29 82	Rain	Rain
24	46	43	50	38	29·38	Fair	Ditto
25	43	50	52	47	29·69	Rain	Ditto

* High wind, hard rain at midnight

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

DECEMBER, 1808.

ARTICLE I.

Answer to Mr. BARLOW'S *Remarks on the Essay on Polygonal Numbers. By* J. GOUGH, *Esq.*

To Mr. NICHOLSON.

SIR,

MY answer to Mr. Barlow's criticism on the solution of Fermat's theorem was in the possession of that gentleman, I believe, prior to the date of his letter inserted in your Journal, Vol. XXI, p. 118. As Mr. Barlow does not think proper to make use of my permission to publish the reply, I am under the necessity of repeating in your present number arguments, which have been already stated in a private correspondence. Answer to Mr. Barlow's criticism.

Mr. B. opens his criticism by admitting the first three propositions, with their corollaries, to be correct; but he does not see in what manner they are to be applied to the general demonstration. This objection may be answered thus: If the remaining propositions be derived from these three, or any one of them, the necessity of inserting them all is established, because the third is derived from the second, and the use of the first appears in the course of the essay. 1st objection answered.

essay. That the propositions following the third are derived from those which precede them is a fact, that is proved by the references; consequently, if my paper contain a general demonstration of the theorem proposed, the necessity of the first three propositions is proved.

2d objection answered.

Mr. Barlow's second objection charges me with false logic: and this gentleman states a sophism, which he considers to be similar to the argument used in cor. 2, prop. 4, of the essay on polygonal numbers. He observes, "that "the author of this essay might, with as much propriety, "have said, that every natural number is either even or "odd, and every aggregate of polygonals being also either "even or odd, therefore every natural number is the aggregate of polygonals." Mr. B. rests his refutation of the argument used in cor. 2, prop. 4, on the supposed similarity of it and the preceding sophism; if then I can show these two to be dissimilar, his second objection must be pronounced futile. To do this, I may observe, that numbers, like most other things, are aggregates of qualities, not single qualities, otherwise there could be no more numbers than qualities; that is, a number, beside being odd or even, is prime or composite, rational or irrational. This consideration shows the nature of the intended fallacy contained in the preceding sophism; for it maintains two aggregates of qualities to be the same; because they have one of these qualities in common. This I presume is an objection, to which the demonstration in question is not liable: for equality constitutes identity in numbers; that is, if any one of two or more equal numbers possess any three of the qualities pointed out above, or any of the properties contained in the definitions to the 7th Book of the Elements, all the rest of them possess just the same, neither more nor less (by axiom 1st of the same book). Now it is shown in the first corollary to the 4th proposition, that every aggregate of polygons of the denomination m is of the form $p + \overline{m-2}.s$; where p is limited by 0 and $m-3$; and s is indefinite: hence it follows, that each aggregate of such polygons is equal to an assignable value of $p + \overline{m-2}.s$. Moreover it appears from the second corollary to the same proposition,

proposition, that every natural number is of the form $p + \overline{m-2} \cdot s$, limited as above; where s may be found, the number being given with m and p; but this value of s substituted in the form $p + \overline{m-2} \cdot s$, gives an aggregate of polygons of the denomination m, which is true in all cases; it is therefore a universal truth not admitting of one exception. The preceding facts appear to give indisputable accuracy to the following syllogism: every natural number is equal to an assignable value of the form $p + \overline{m-2} \cdot s$; and there is an aggregate of polygons of the denomination m equal to the same value of the same form; therefore every natural number is an aggregate of such polygons; because things, which are equal to the same thing, are equal to one another; Euclid, Axiom 1, Book 1; and equal numbers have been shown to have the same qualities neither more nor less. The supposed similarity betwixt my critic's sophism and the preceding mode of argument appears to be done away; for he proceeds on the supposition, that the sameness of one quality constitutes identity in numbers; but the first axiom of the 7th book of Euclid is the foundation of my reasoning; namely, that a perfect agreement in qualities produces the same thing, namely, identity of numbers. My opponent, in fact, does not rely altogether on the similarity of his intended, and my accidental sophistry; for he produces a second sophism, and pronounces it to be strictly analogous to mine, though it differs in every particular from his former parody of my supposed mistake. Mr. B. observes, that "every natural number is of the form "$p + \overline{m-2} \times s$; and, every square number being also of "the form $p + \overline{m-2} \cdot s$, therefore every natural number is "a square number." It is true, that every square number is of the form $p + \overline{m-2} \cdot s$; but then s is limited, being of the form $s = \frac{q^2 + 2qv + v^2 - p}{m-2}$, where $q^2 = p$ or the next greater square when p is not a square, and v is to be taken so as to make s a whole number; but s is unlimited in the case of natural numbers; therefore, by the rules of logic, every square integer may be proved to be a natural

number

number, but not every natural number a square integer. On the contrary, s is unlimited in the aggregates of polygons, as well as in natural numbers; therefore, my opponent's second parody is equally unsuccessful with his first; because its imaginary resemblance to my syllogism has been shown to be spurious. In the conclusion of this objection it is remarked, that the corollary under consideration might be assumed as a postulate, i. e. as a self evident problem; but far from treating it either as postulate or axiom, he agrees with me in giving it the importance of a theorem, and demonstrates it accordingly.

3d objection answered.

Mr. Barlow's third objection is occasioned by an obvious mistake on his part: I have said, that, if $e = y + t$ can be resolved into $m - f$ polygons, $e + f$ may be resolved into m polygons of the same denomination. My critic puts a construction on this expression, which makes me suppose, that $y + t$ can be resolved into $m - f$ polygons of the denomination m, in all cases. This is evidently a misconception; for, had my opinion agreed with Mr. Barlow's interpretation of it, why have I attempted to demonstrate the theorem of Fermat; the truth of which I am supposed to assert without demonstration in the preceding quotation from my essay? The genuine meaning of the passage is obviously this; if $e = y + t$ can be resolved into $m - f$ polygons in any one case; $e + f$ may be resolved into m such polygons in the same case; which construction of the expression refutes this part of the criticism.

The proposition shown to be universal.

All my opponent's objections have now been controverted; but he farther remarks, that there is a difference betwixt doing a thing, and proving that it may be done in all cases. The justice of this observation obliges me to show, that my essay also contains the principles of the latter demonstration. For this purpose let the reader look at example 2, prop. 7, which will assist him in the following reasoning. If $e = y + m - 1$, it may be resolved into m polygons (by cor. prop. 6): again, if $e = y + m$, it may be resolved into two polygons, which are less than m; here $f = m - 2$, and $e + f = y + 2m - 2$ is resolved into m polygons, (cor. prop. 6): moreover, if $e = y + 2m$, it consists of three polygons (by prop. 6, and cor. 1, prop. 2), but three is the least

Radius of Curvature

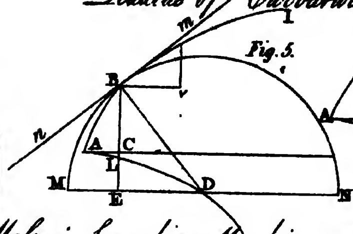


Fig. 5.

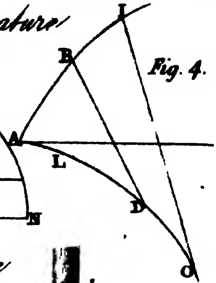


Fig. 4.

Massey's Sounding Machine

Fig. 1. Massey's Patent Log

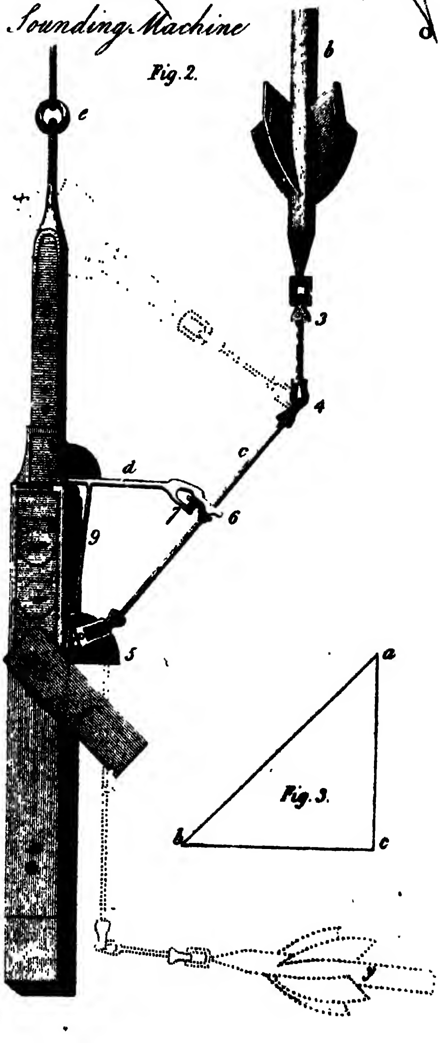


Fig. 2.

Fig. 3.

least value of m; hence all the numbers from y to $y + 2m$ are properly resolved, except $y + 2m - 1$. Now, let a be the index, which resolves $y + 2m - 2$ into polygons; and the same, a, will resolve $y + 2m - 1$ into $m + 1$ polygons; but the next value of $a = a + 2 - m$ (cor. 1, prop. 4); which will resolve $y + 2m - 1$ into m polygons, or less, (cor. 2, prop. 5). In general, if $e = y + t$ can be resolved into m polygons by the index a, the next index $a + 2 - m$, will resolve $e + 1$ into m polygons, or less (cor. 2, prop. 5).

JOHN GOUGH.

Middleshaw, October 15, 1808.

II.

Description and Use of a Sea Log, and Sounding Machine, invented by Mr. EDWARD MASSEY, *of Hanley, in Staffordshire.*

TO the nautical reader the advantages resulting from a log, that will give a dead-reckoning free from errour, or nearly so, must be sufficiently obvious; and to others it would be superfluous to point them out. The principle, on which Mr. Massey's patent log is constructed, is not new; but every application of it to practice has been found defective, and this is the difficulty the patentee has had to surmount. To understand the manner in which it acts, see Pl. 7, where a, fig. 1, is that part of the log which registers the distance sailed, and is therefore called the register; it contains within itself a set of wheel work, which operates upon the fingers of the several indices, 1, 2, and 3. b is the rotator, a hollow cylinder, made air-tight, and so nearly of the same specific gravity as water, as to float when drawn forwards with the velocity of mere steerage way. On this rotator are fixed four vanes placed obliquely. It is then fastened to the register by a cord, c, about six feet long*: to the loop-hole

An accurate log wanted.

New log described.

* This cord is shown scarcely one tenth part of its proper length in the engraving: it would have been an unnecessary extension of the plate to represent it otherwise, as it may so readily be conceived.

at

at the other end of the register is secured another line, *e*, of sufficient length to extend beyond the eddy of the vessel's wake.

The finger on the index 1 revolves once while the log moves forward one mile; that on the index 2 moves once round in going ten miles; that on the index 3 makes one revolution when the distance sailed is one hundred miles. When the machine is to be used, all the fingers of the indices are set to 0, and both register and rotator committed to the water.

Its mode of action.

As the vessel moves forward, the log must follow, and from the obliquity of the vanes it is evident the rotator, *b*, must revolve quicker or slower, correspondent to the ship's velocity. This rotatory motion is communicated by the cord *c* to the universal joint *d*, connected with the wheels, which consequently revolve with the rotator and cord, and thus the actual space passed through, in any given time, is registered on the indices.

Registers the whole distance sailed.

Every occasional or momentary acceleration or retardation of the vessel, from irregularity of wind, or other causes, which are either altogether passed over, or very vaguely guessed at, in general, are accurately registered on this machine, which not only gives the actual rate of sailing, but the actual distance sailed, since the last inspection.

A very little reflection will convince any observer of the great superiority of this machine over all others which have been hitherto introduced.

Former attempts have failed,

It may appear rather presumptuous to criticise the labours of Smeaton, and many others, whose endeavours were not crowned with complete success: but it is necessary to point out where their plans failed, in order to prove the very superior advantages of Massey's log; for though some of the machines answered their purposes tolerably well under certain circumstances, none of them were nearly correct under all circumstances. Some were erroneous when the ship moved less than four miles in the hour, and others became so when the rate was increased.

and why.

In most of the former inventions, the first mover was a spiral, or a rotator in the shape of a Y, and was generally attached to a register kept in the ship; and as it was absolutely necessary, that this first mover should be out of the

wake

wake of the vessel, it had a length of fifty yards of cord, or more, to carry round with it every time it revolved. The friction caused by this operation was such as to preclude all hopes of accuracy; it was useless in an agitated sea, the rope was very liable to kink, and in fast sailing the rotator would sometimes fly out of the water*. These circumstances rendered it impossible, that the rotator should make the same number of revolutions in passing through a given space, under different velocities; and hence inaccuracy was unavoidable. To get rid of this friction of the long line, the rotator has, in some instances, been enclosed in a cylinder, and a register been attached to the outside. But though the defect of excessive friction was thus surmounted, still greater inconveniences resulted. It may be sufficient to mention, that the cylinder, not presenting itself horizontally in the water, was liable to alter its position whenever the velocity of the vessel was changed, which caused an eddy, or dead water, to remain in the cylinder; and, of course, the rotator was liable to err, in proportion as the cylinder lost its horizontal position.

After thus hinting at the imperfections of other previous methods of constructing logs, it remains to point out wherein Massey's plan differs.

Difference of the present log.

Friction is the principal cause of mechanical theories varying so widely from actual experiment. In some machines one third is allowed for its effect, while the operation in others is nearly suspended, and what appeared very plausible in theory, is found totally useless in practice. Thus the friction on a rope long enough to extend beyond the eddy of the vessel's wake would, in many circumstances, on the old plan, totally impede the action of the rotator†. Under this impression,

Friction trifling

* Smeaton, in the account of his experiments on Saumarez's log, in the Philosophical Transactions, observes on this subject "Upon making up the account of this run, I found the number of rotations were "less by one full third than they ought to have been, compared with "the former observations, which afforded me a convincing proof, that "this instrument was considerably retarded in quick motions"

† Smeaton, in the account of his experiments in the work before quoted observes: "During this run, I observed that the resistance of "the water to the line and plate was very considerable, and increased the "friction

impression, the friction in Massey's patent log is reduced to almost nothing by the following simple contrivance. The whole log, consisting of the register, *a*, connecting cord, *c*, and the rotator, *b*, is committed to the water, by a log line of sufficient length to reach beyond the eddy of the vessel's wake. As the ship moves forwards, the rotator, and cord, *c*, between it and the register, revolve and set the wheels into motion; nor has the roughest sea been found to prevent this action.

Rotator always horizontal,

The rotator also, in this log, is so constructed as always to preserve a horizontal position, by being made nearly of the same specific gravity as water; which is effected by means of an air tube passing through its centre: an indispensible requisite, which no former machine possessed; and for the want of which, they could not preserve that horizontal position in fast and slow sailing, which is absolutely necessary to obtain any true result.

and accurately adjusted.

Another very important improvement consists in the contrivance for regulating the rotator, by which means every rotator revolves once on its axis in passing the same space: as it was found utterly impossible to construct two rotators so exactly alike as stated by Smeaton, without means of adjustment.

General properties of this log.

To sum up the properties of Massey's patent log, in a few words it may be observed,

1. It will give the true distance sailed, from steerage way, to any velocity with which the swiftest sailing vessel can move.

2. It not only gives more accurately than the common log the rate of sailing, but the actual space sailed through since the last inspection.

3. It is attended with less trouble than the common log, and no mistakes can possibly arise from the result it gives.

Another advantage it has in giving us a

It remains to point out one great and desirable advantage, which may very reasonably be expected to result from the use of this log, and that is, a more complete knowledge of

"friction of the spindle so much, as to prevent it from beginning to "turn, till the plate had twisted the line to such a degree, that when it "did set a going it would frequently run one hundred and fifry or two "hundred turns at once."

the

the currents in various parts of the ocean, which has hitherto been very imperfectly attained; as it was not possible to know, with any certainty, whether the wide difference found between the real distance, and that given by the common log, was caused by the known imperfections of that method of reckoning, or by the operation of currents.

knowledge of currents.

Dr. Maskelyne, in the same work just quoted, further observes: "There is another argument which adds much "strength to the foregoing ones, and greatly enforces a "uniform and correct length of the logline, on board all "ships; that in many parts of the ocean, especially be-"tween the tropics, and near most head-lands, there are "considerable currents, which must introduce a fresh er-"rour into the reckoning; and if this errour should happen "to combine with that already produced by a wrong length "of the logline, as it may as well as not, it is not easy to "say how far the total errour of the reckoning might go, "or to what inconveniencies or dangers the ship might be "exposed on that account. But if the just and proper "length of the logline were used on board of all ships, "they would be then liable only to the errours of the cur-"rents themselves; and even these, as far as they are con-"stant and regular, might be found out and ascertained, "from the journals of several ships, which would then agree "much nearer with one another." And Smeaton observes, "that it is for want of a means of measuring the way of a "ship through the water, (and this compared with other "check observations,) that the drift and velocities of the "principal currents have not already been determined."

Remarks on this by Dr. Maskelyne

and Mr. Smeaton.

But admitting the common logline and glass were perfectly uniform in each ship of a fleet, yet the result would still be too erroneous to expect this very desirable knowledge of the currents to be derived from a comparison of the several journals. Massey's patent log holds out, however, more than a probability of effecting this important end. It appears by a letter from Captain Whittle, of the Lord Nelson, that he found the distance run from the island of Ila, to St. John's harbour, Newfoundland, by Massey's log, to agree with the known latitudes and longitudes of both places, within eight miles. Now had he sailed in company with several

Reckoning to Newfoundland kept true to eight miles.

veral other ships, supplied with the same log, which had kept tolerably well together during the whole voyage, and it had been found (which is more than probable) that all their reckonings corresponded with his; the difference between the true distance, and the distance given by the log, might with the greatest propriety be ascribed to the operation of currents; the existence of which would consequently be discovered, as far as related to those seas.

From the common method of taking soundings many ships lost.

The importance of obtaining true soundings at sea must be admitted by every seaman; and it is rather singular, that no other method than the common lead has hitherto been brought into use; as its imperfections are very generally acknowledged.

Causes of its inaccuracies.

Many vessels have been lost, by depending upon the soundings taken in the usual way. The difficulty of obtaining the true perpendicular, and the uncertainty as to the exact moment when the lead strikes the bottom, upon which the accuracy of the result depends, must always prevent the possibility of obtaining the true depth, while the ship has any considerable way upon her. Indeed, it has been acknowledged by experienced seamen, during some experiments, made at various times, in the river Mersey, that they could not depend upon the common lead, when going five or six knots in the hour, in ten or twelve fathoms of water. When the depth is considerable, the vessel must be hove to, which is an operation attended with great loss of time, and sometimes considerable injury to the sails; and during a chase, this inconvenience must be particularly felt.

New method.

Massey's sounding machine is as great an improvement upon the common lead, as his patent log is upon the common log. A rotator on the same principle as that to the log registers the perpendicular descent of the lead, without any respect to the length of line paid out, which, in the usual method of taking soundings, is the chief guide to the mariner in judging of the perpendicular depth, and is apt to deceive him much.

Soundings taken in 30 fathoms without heaving to.

True soundings may be taken with this machine in thirty fathoms water, without the trouble of heaving the vessel to, although she may be going at the rate of six miles in the hour. True soundings may also thus be obtained in very deep

deep water, where it is not possible to take them by the common lead.

This sounding machine is on the same principle as the log, for it is evident, that, if the end *e* of the register, *a*, (*fig* 1) were projected into the water, and suffered to descend, the rotator would follow, and register the exact depth, as well in a perpendicular, as in a horizontal position. Principle of the machine.

But though the principle of the two machines is the same, their construction necessarily differs considerably, as will be perceived on reference to the plate.

Fig. 2 represents the sounding machine. *a* is the sounding weight, containing a register, 1, 2, with two dials: the hand of the dial 1 makes one revolution when the weight has descended twenty fathoms, the other revolves once when the descent amounts to five hundred fathoms. A rotator, *b*, similar to that attached to the log, communicates with the wheel work of the dials 1, 2, by means of the rod *c*, on which there are three universal joints, 3, 4, and 5. This rod is supported during the descent of the weight, by the drop, *d*, at the end of which is a fork, 6, and a friction wheel, 7. Description of it.

When the machine is to be used, a sounding line is fastened to the ring, *e*; and one of the vanes of the rotator is slipped into the spring 8: the rotator will then be in the position indicated by the dotted lines, *x*. The indices must be set at 0, and the cover or lid, *f*, be shut. The machine must then be projected perpendicularly into the sea. As soon as it reaches the surface, the resistance of the water forces the dotted rotator, *x*, out of the spring 8, and it assumes its perpendicular direction as represented by the rotator *b*. As the machine descends, it is evident the rotator will revolve, and its motion be communicated freely past the friction wheel 7, and the universal joint 5, to the wheel work of the dials 1, 2, and thus indicate the space passed through in fathoms. When the machine has arrived at the bottom, the rotator, as it is no longer buoyed up by the reaction of the water, will fall to the bottom, quitting the fork of the drop *d*, which will also fall from its horizontal position, and in its descent, by means of the locking rod 9, prevent the rotator from revolving as the machine is drawn up. When at the bottom, Method of using it.

bottom, the rotator will be in the position of the dotted lines y.

No mistake can arise from it.

This machine, simple in its construction, and scarcely more liable to accident than the common lead, ascertains, with the utmost precision, the perpendicular depth, by the mere act of descent through the water. No mistake can arise from that common source of errour, the drift or lee-way of the ship during the time of descent; nor does an operation of such importance depend upon the uncertain sensation caused by the lead striking the bottom, on which the accuracy of the common log altogether depends, and which, it is well known, frequently and materially misleads the best seaman; for though a thousand fathoms of line were paid out, in the smallest depth of water, no inaccuracy could arise, as the perpendicular depth, at the point of heaving, would be registered on the index. The only inconvenience experienced would be the additional labour necessary for hauling in the excess of line. The most inexperienced person may use this machine, without risk of errour, in the most turbulent sea, and during the night.

The advantages already enumerated would render the sounding machine of great importance; but there are other properties of still more consequence.

Farther advantages.

To heave a ship to, in order to obtain soundings, on a lee shore, in stormy weather, is a very disagreeable operation, attended with much trouble, and loss of way; also with considerable danger to the ship's sails; indeed, it would often, under such circumstances, be attended with great hazard to the safety of the ship. To avoid these unpleasant consequences, the master sometimes adopts a measure, which he conceives to be the less exceptionable alternative, by running on without sounding at all.

Sounding in 60 or 80 fathoms while going 3 knots an hour.

To prove how much inconvenience and danger are avoided by Massey's lead, it is enough to state, that soundings may be taken in depth from 60 to 80 fathoms, while the ship is under way, at the rate of three miles an hour; and as the rate of sailing may be still materially reduced, without entirely stopping the vessel, or altering her course, so may soundings be had, to any depth required, while she is under way.

In

Its superiority exemplified.

In order more clearly to show the superiority of this machine, and make it apparent, that the quantity of stray-line veered out does not at all affect the truth of the result: suppose the common lead thrown from the mizen chains of the ship, which may be represented by the point *a* of the triangle *a b c*, (fig. 3), and that the ship has moved forwards through the space equal to the line *b c*, while the lead has descended through the line *a c*; it is evident, that it is impossible, in this case, to ascertain the exact depth, as a quantity of line, equal to *a b*, would be paid out, whereas the true depth is equal only to the line *a c*, which is much less. But the case is very different when the patent sounding machine is used, as the operation ceases when it has reached the bottom; nor is the stray-line, *a b*, whatever its length, at all taken into the account.

Takes accurate soundings at any depths.

It has been found extremely difficult, and sometimes impossible, to obtain soundings in very deep water with the common lead, which may perhaps be thus accounted for. The common line which is used for sounding, though, if left to itself, it would sink in water, yet its descent would be much slower than that of the lead, separately; it consequently follows, that the lead must be so much impeded by carrying the line with it, that when it does reach the bottom, there will be scarcely any sensible check to enable the seaman to know the precise moment. Indeed, if he can ascertain even this to a certainty, he still cannot depend upon the truth of his soundings; for if there be the least drift or current, the line itself will assume a curve, similar to that of the line of a kite in the air. These two causes will always operate against the perfection of the common mode of sounding.

After so fully describing the principle of the patent sounding machine, it is scarcely necessary to prove, that it is liable to neither of the foregoing objections; and it may be sufficient to say, that, as it will certainly find its way to the bottom, if a sufficient portion of stray-line be allowed to guard against its being checked in its progress, and the certainty of its having reached the bottom may be ascertained by the arming, there can be no doubt of the practicability

ticability of its obtaining soundings, in any depth, and no reasonable doubt of their correctness when obtained.

The rotator does not impede its descent,

From the construction of this machine, it might be imagined, that the rotator would impede its motion through the water, and that it could not descend so rapidly as the common lead; but during repeated trials, in thirteen fathoms water, in which the rotator was frequently detached, and the lead suffered to descend alone, there was no difference perceptible in the time of their descent, though an excellent quarter-second stop watch was used during the experiment,

as shown by experiments.

to detect any change. The following table shows how very uniformly the times of descent corresponded with the depths in fathoms, during a series of trials made on the river Mersey, with the patent lead, weighing fourteen pounds.

The manner of conducting these experiments was such as is deserving of perfect reliance. Two pilots, of well-known ability and experience, were employed: one threw the lead, and the other, the moment he found, by the slackening of the rope, that the weight had arrived at the bottom, cried, 'stop,' to a third person who held the watch.

Time of descent.		Fathoms.	Time of descent.		Fathoms.
2	seconds	2½	7¼	seconds	11½
2½	———	3	7¼	———	11½
3	———	4	7¼	———	11½
5	———	8	7½	———	12
5½	———	8½	7¾	———	12¾
6	———	10	8	———	13
6	———	10	8¼	———	13½
7	———	11¾	6	———	10

Taken when under sail, at upwards of five knots in the hour.

Several captains and masters in the navy have made trial of the log and sounding machine, and given very favourable reports of their performance. Of these the two following may be selected as specimens.

San

San Josef, 12th Dec. 1806.

Having several times, and in different depths and rates of sailing, tried Mr. Edward Massey's patent sounding machine, which is, in my opinion, a most excellent invention, as correct soundings were gained in fifty-five fathoms, with a strong breeze, going six knots, by only passing the lead to the quarter-boat, attaching a hand lead about thirty fathoms from the machine, (which I think, is in such cases necessary :) and about ninety fathoms of line out: at another trial, to compare the old with the new method, going five knots and a half, correct soundings were ascertained by the machine in fifty-two fathoms, by passing the line to the main-chains, when we could barely get the depth in the old way, by carrying the lead to the spritsail-yard, notwithstanding the immense length of a firſt rate, and daylight in our favour; and not even then, if we had not had knowledge of the depth nearly, that being a check or caution not to give too much line off the reel, there being no time to gather in the slack, which would be the case were we sounding in an unknown place, by the old method. The invention is the more valuable, as the process is the most simple, the whole being understood, by seeing it once in use.

Testimony of the performance of the sounding machine,

I therefore consider it a valuable improvement in navigation; as in frequent, and various cases, soundings could not be gained without it. The advantages are many, such as in chase, or being chased; on a lee-shore, or doubtful of it; and to save time in running for the desired port*.

R. J. NEVE, Captain.

N. B. It will be necessary in the practice of the new method of sounding, to have line of different sizes, in proportion to the depth of water; as by the ship passing at the rate of eighl or ten knots, it will require the best of lines to haul in the lead, and should be made of a much superior quality to those at present supplied to the navy.

* The honourable Navy Board have adopted the sounding machine for the use of his majesty's navy, and have favoured the inventor with an order for five hundred machines.

H. M.

H. M. S. San Josef, in Torbay, 12*th Dec.* 1806.

SIR,

and of the log. In obedience to your orders, we have been particular in attending to the use of Mr. Edward Massey's Patent Log, and from every opportunity that offered during our cruize we are strictly of opinion, that it has the merit of accomplishing the end for which it is intended.

On some trials made with it, and the common log, they perfectly agreed, at other times they differed a little, but last night bearing up for Torbay, with a run of eighty miles in squally weather, there was a difference of nine miles: but agreeably to our reckoning the patent log was perfectly correct; we therefore consider it an important improvement in navigation, and the more so, as the instrument is simple and easy to be generally understood.

The chief things necessary to be observed are to secure the tow-line as near the surface as possible, to prevent the machine from quitting the water in an agitated sea, and fast sailing, and not to be less than sixty fathoms long in a first rate, to prevent it from being affected by the eddy of the ship's wake.

We are, Sir,

Your most obedient humble servants,

R. J. NEVE, Captain.
THOMAS MOORE, Master.

To sir Charles Cotton, bart., viceadmiral of the red, &c.

III.

Observations on the Problem respecting the Radius of Curvature. In a Letter from W. MOORE, *Esq.*

To Mr. NICHOLSON.

SIR,

IF the following observations on the problem respecting the radius of curvature, should be found to deserve a place in your Philosophical Journal, the insertion of them will greatly oblige,

Sir,

Your most obedient humble servant,

W. MOORE.

An attempt to show, that the nature of the problem respecting the radius of curvature does not involve in it the consideration of second fluxions; but that they are made to enter into the definitive expression as a matter of mere convenience.

Problem respecting the radius of curvature does not involve second fluxions.

Definition. If one end, O, of the thread A L D O, Pl. vii, fig. 4, be first fixed to the point O, in a curve L D O, concave the same way, and afterward the thread be put about the said curve so as to touch it in every part: then if the other end, A, of the thread be tightly moved in the same plane with the curve L D O, the said end, A, will describe a curve A B I, called the *involute* curve to L D O which is the *evolute*: and the right lines D B, O I, are said to be radii of curvature.

It is evident from the method of generation of the curve A B I, that if at any point D, in the *evolute* L D O, the string should cease to unwind itself and the radius DB continue to revolve about D, as a centre (see figure 5), the circle thereby described would have the same degree of curvature as the *involute* at the point B; and that a tangent drawn to either curve would be common to both. Moreover, because the said two curves, viz. the *involute and circle*, have the same curvature at the point B, and their continuations one and the same curve, namely, the circle where radius is D B; the fluxions of the absciss and ordinate of the one, will be equal to the fluxions of the absciss and ordinate of the other, and consequently the same will hold of any other order of fluxions whatever. This being premised, let it be required to find a definitive expression for the radius of curvature of any curve, as A B I. For which purpose let as usual the absciss and ordinate A C, B C, of the curve A B I, be denoted by x and y; also A B $=Z$, and put B E the corresponding ordinate of the circle M B N $=v$. Then the triangles *Bvm*, B E D, being similar, we have Bv : Bm :: BE : BD; or $\dot{x} : \dot{z} :: v : \text{BD} = \frac{v\dot{z}}{\dot{x}}$. Now this expression is general; but being in terms of the ordinate of the circle M B N and unequal to B C, the ordinate of the circle; it cannot with convenience be applied to curves whose equations are generally expressed in terms of their

Problem respecting the radius of curvature does not involve second fluxions.

own ordinates: we must therefore in order to adapt it to practical purposes find a value for v, in such terms as shall be consonant to the characters in which the equations of curves are generally written, viz. those expressing the absciss and ordinate: in order to which, let the above expression be put again into fluxions, and made equal to nothing (it being a constant quantity) and we shall have $\frac{\dot v\dot z+v\ddot z}{\dot x}=\frac{v\ddot x\dot z}{\dot x^3}=o$; and $v=\frac{-\dot v\dot z\dot x^2}{\dot x^2\ddot z-\ddot x\dot z}=\frac{-\dot y\dot z\dot x^2}{\dot x^2\ddot z-\ddot x\dot z}$; therefore, $BD=\frac{v\dot z}{\dot x}=\frac{\dot y\dot x\dot z^2}{\dot x^2\ddot z-\ddot x\dot z}$, another general expression in terms of the fluxions of the arc, absciss and ordinate of the involute—but this, like the other is still inconvenient for practice; yet the difficulty may be removed very easily by expunging $\ddot z$: for put $\dot x^2+\dot y^2=\dot z^2$ into fluxions, and we get $\dot x\ddot x+\dot y\ddot y=\dot z\ddot z$; and $\ddot z=\frac{\dot x\ddot x+\dot y\ddot y}{\dot z}$ so that our last expression becomes $BD=\frac{-\dot y\dot x\dot z^2}{\dot x^2\ddot z-\ddot x\dot z}=\frac{-\dot y\dot x\dot z^3}{\dot x^3\ddot x+\dot y\ddot y\dot x^2-\ddot x\dot z^2}=\frac{-\dot y\dot x.\ \overline{\dot x^2+\dot y^2}^{\frac{3}{2}}}{\dot x^3\ddot x+\dot y\ddot y\dot x^2-\ddot x\dot z^2}$; the general value for the radius of curvature, when both the absciss and ordinate flow inconstantly: but as all curves may be generated, either by the uniform increase of the absciss and inconstant variation of the ordinate; or by the uniform flow of the ordinate and variable flux of the absciss; we are at liberty to assume the first fluxion of either constant as it may suit our convenience; and thus simplifying the expression, avoid the trouble which would otherwise arise.—Thus, if $\dot x$ be supposed constant, the expression will be $\frac{\overline{\dot x^2+\dot y^2}^{\frac{3}{2}}}{-\dot x\ddot y}$; and if $\dot y$ be made constant it becomes, $\frac{-\dot y\dot x\ .\ \overline{\dot x^2+\dot y^2}^{\frac{3}{2}}}{\dot x^3\ddot x-\dot x^2\ddot x-\dot y^2\ddot x}=\frac{-\dot y\dot x}{\ddot x}\times\frac{\overline{\dot x^2+\dot y^2}^{\frac{3}{2}}}{\dot x^3-\dot x^2-\dot y^2}$.

It is to be remarked, that all those expressions for the radius of curvature are strictly true and general; yet being in terms of quantities whose values would be extremely difficult to find, are not so applicable to practice as that containing only the fluxions of the absciss and ordinate. The entry of second fluxions into the definitive expression, does

does not imply, that the *nature* of the problem necessarily requires it: it arises from the particular artifice which is employed in finding the value of v, the ordinate of the circle; and is a matter of mere commodiousness, suggesting no other reason for their appearance, than that of a necessary consequence of such a particular step.

Royal Military Academy, Woolwich,
Oct. 13*th*, 1808.

IV.

Essay on the Composition of Alcohol and of Sulphuric Ether. *By* THEODORE DE SAUSSURE.

(*Continued from p.* 231.

SECT. III. *Analysis of alcohol by detonating its vapour with oxigen gas.*

The whole of the alcohol not burned in the preceding experiments.

IN the preceding analysis I remarked, that alcohol, burning in a lamp under a closed receiver, diffuses a vapour, that has an alcoholic smell; it is very probable therefore, that the whole of the combustible disappearing from the lamp does not burn. Accordingly I sought a process, that should effect a complete combustion of the alcohol; and this I found in detonating a mixture of vapour of alcohol and oxigen gas over mercury, by the electric spark, in Volta's eudiometer.

Its vapour detonated.

This a difficult process.

This process applied to the analysis of alcohol is somewhat complex. It requires a knowledge of the weight of the vapour of alcohol at a given temperature and pressure, and the determination of the increase of volume of the oxigen gas by the presence of the vapour. The experiment must be conducted at a temperature exceeding 15° R. [66° F.] to obtain sufficiently decisive results; and neither the thermometer nor the barometer must vary during the course of it, which requires practice and quickness in several of its manipulations.

Method of computing the dilatation of vapour of alcohol.

I washed the inside of a large bladder with alcohol several times, letting the alcohol stand in it a long time, to take up every thing soluble in it, that this might not affect the expansibility. When this came out perfectly pure, the bladder was three parts filled with atmospheric air, two ounces of alcohol were poured in, and it was stopped with a cock. The air contained in it was dilated by the formation of alcoholic vapour. At the expiration of eighteen hours I fitted to the cock an empty receiver intended to weigh the air. The cock being turned, the dilated air passed alone, without any liquid alcohol, into the receiver, which was weighed before and after, the thermometer being at 17° [68° F.], and the barometer at 26 inches 9 lines during these operations and those that followed.

Twice repeated.

By this experiment, repeated twice under these circumstances, I found, that 1000 cubic inches of atmospheric air dilated by alcoholic vapour weighed 433·78 grains; and 1000 inches of the air employed in the experiment weighed before the introduction of the alcohol 424·5 grains.

Dilatation of the gas calculated by Mr. Dalton's formula.

To measure the dilatation the air had undergone by the alcoholic vapour, I employed the formula of Mr. Dalton, and passed into a barometer a drop of alcohol, which sunk the barometer 20·5 lines, expressing the elastic force of the vapour in vacuo. Applying this result to the formula $\frac{p}{p-f}$, where $p = 26$ inches 9 lines, and $f = 20·5$ lines, we find, that, the volume of undilated air being equal to 1, it becomes 1·0682 by the conversion of alcohol into vapour; and as 1·0682 : 1 :: 1000 : 936·14, it may be inferred, that 1000 cubic inches of atmospheric air alcoholized contain 936·14 of atmospheric air. These weigh 397·4 grains; and as the alcoholic vapour occupies the same space as the air dilated by it, it follows, that 1000 cubic inches of pure vapour of alcohol weigh 433·78 — 397·4 = 36·38 grs.

1000 cub. inches of alcoholic vapour weigh 36·38 grs.

Vapours diffuse themselves in equal quantity whatever the gas.

I need not remind the reader, that, according to Dalton's experiments, vapours diffuse themselves in the same quantity through every gas, that has no chemical action on them*. I chose atmospheric air for finding the weight of the

Atmospheric air very slowly altered by alcohol.

* I kept atmospheric air in contact with alcohol a long time in a jar over mercury. In five months the air had undergone no sensible change, but in twelve it had lost ·01 of oxigen gas.

the vapour, because I could not employ pure oxigen gas in large quantity otherwise than at the point of extreme moisture; and if it had got dry, or if the external air had any way penetrated into the bladder, there would have been some errour in the calculation of the weight. I repeated the experiment however with oxigen gas, and found only a trifling difference, which I ascribe to the causes just mentioned.

Alcoholic vapour mixed with oxigen gas

To effect the combustion of alcoholic vapour, I prepared alcoholized oxigen gas, by passing some drops of alcohol into a jar filled with oxigen gas over mercury. I afterward withdrew the superfluous alcohol, that could not rise in vapour, by introducing dry unsized paper, and taking it out through the mercury, repeating this operation till the paper came out perfectly dry, and then emptying the dilated gas into a fresh jar, I had previously satisfied myself, that unsized paper would not condense the vapour of alcohol.

would not detonate by the electric spark

This alcoholized oxigen gas was introduced into a Volta's eudiometer filled with mercury, but I could not set it on fire by the electric spark. I was equally unsuccessful on adding pure oxigen gas in various proportions. The alcoholic vapour was too much rarefied in the oxigen gas to take fire.

without a mixture of hidrogen gas, or a little liquid alcohol.

When I added a very small portion of hidrogen gas to the alcoholized oxigen gas, the electric spark produced complete combustion of the alcoholic vapour. The same effect took place, when I substituted an inappreciable quantity of liquid alcohol instead of hidrogen gas. The vesicular vapours, produced no doubt by this alcohol, answered the purpose of hidrogen gas: but in an accurate experiment this addition of liquid alcohol was inadmissible, as it was impossible to ascertain its quantity.

The experiment described.

Accordingly to 500 parts by measure of alcoholized oxigen gas I added 99·2, or near a fifth, of hidrogen gas, and detonated the mixture. The combustion, taking a mean of three experiments, gave a residuum, which, being analysed by lime water and by Volta's eudiometer, contained 342·59 parts of oxigen gas, and 46·69 of carbonic acid gas. I omit the nitrogen, which was found mixed in a small quantity with the oxigen gas both before and after the combustion, and acts no part that can be estimated. I must observe, that, when I opened the eudiometer immediately after the detonation,

tion, and while it was full of fumes, these were perfectly void of smell.

Calculation of the products.

The 500 parts of alcoholized oxigen gas contained before the combustion, according to the expansion of alcoholic vapour, only 468·07 parts of oxigen. The alcoholic vapour therefore and hidrogen gas added occasioned the disappearance of 468·07 — 342·59 = 125·48 parts of oxigen gas. The hidrogen gas added condensed half its bulk, or 49·6 parts. The 500 parts of alcoholic vapour therefore employed in their combustion 125·48 — 49·6 = 75·88 parts, forming 46·69 parts of carbonic acid gas, and a certain quantity of water.

If we consider the parts above mentioned as cubic inches, and to 500 of these substitute their equivalent weight of alcohol, we find, that 18·19 grs, of alcohol consume in their combustion 75·88 cubic inches of oxigen gas, forming some water, and 46·69 cubic inches of carbonic acid gas. These results by a similar calculation to that made for the slow combustion of alcohol, sect. II, show that 100 parts of this liquor contain

Elements of alcohol.

Carbon	48·82
Hidrogen	15·82
Oxigen	41·36
	100.

These elements may be deduced from the following expression. Ten grains of alcohol consume for their combustion 38·54 cubic inches of oxigen gas, the thermometer being at 28 inches and the thermometer at 10° R. [54·5° F.], forming water, and 23·67 cubic inches of carbonic acid gas.

This analysis more accurate than the former.

This analysis, in which the whole of the alcohol was consumed, must be more accurate than that made by slow combustion, sect. II. I shall presently show, that a small quantity of nitrogen is to be included in both.

Sect. IV. *Examination of the water produced by the combustion of alcohol.*

Alcohol in burning forms water.

Boerhaave and Geoffroy observed, that the vapour formed by the combustion of alcohol was water. Lavoisier found

found by means of an apparatus invented by Meusnier*, that the weight of this water exceeded that of the alcohol consumed. In this process all that is formed is not collected, because this process is conducted in open vessels, in which the air is continually renewed by a rapid current, that carries out of the apparatus a considerable portion of the vapour before it has time to condense. In burning 100 parts of spirit of wine Lavoisier collected about 116 parts of water†. My analysis, sect. III, shows, that this aqueous product should amount to 132 parts for 100 of perfect alcohol: but Lavoisier did not employ this, which would have afforded a result nearer to mine. As it is impossible to make this comparison with accuracy, I contented myself with examining whether the water produced by this process were pure.

100 p. alcohol produce 138 water.

The water examined.

The water obtained from alcohol by the apparatus of Meusnier, or more simply by burning it in the open air under the mouth of a large glass receiver, which condenses the aqueous vapours on its sides, so that they drop from its mouth, has not the alcoholic smell observed in the product of combustion under a close receiver, sect. II; because in the latter the alcoholic vapour is retained, while in the open air it is dissipated, leaving as a residuum only the less evaporable fluid with which it was mingled.

Its properties.

This liquor is insipid: it has the same specific gravity as distilled water: it does not change the colour of sirup of violets or of infusion of litmus: it is not precipitated by acetate of barites, nitrate of silver, or limewater.

Residuum left by it.

Two ounces of water obtained from the combustion of alcohol in the open air under the mouth of a glass receiver were evaporated to dryness, and left as a residuum a thin transparent varnish, that weighed ¼ of a grain, and attracted moisture from the air. The solution of this varnish in a small quantity of water was rendered slightly turbid by oxalate of potash. The combustion of spirit of wine rectified without addition afforded the same result. This residuum

The same by alcohol rectif-

* For a description of this apparatus see Lavoisier's Elémens de Chimie, vol. II, p. 189, 1st. edit.

† Lavoisier's (posthumous) Mémoires de Chimie, vol. II, p. 281.

appeared

ed without addition. appeared to me owing in part to the lime and potash, which I have found in the ashes of alcohol by other experiments. They are held in solution by acetic acid formed by the combustion. This water, kept in a phial half filled with it, after some months deposited a slight mouldiness.

The water grew mouldy.

Muriatic acid elicits ammoniacal vapours from it.

At the approach of muriatic acid this fluid diffuses copious ammoniacal vapours. This effect is more striking, when the water has been collected by Meusnier's apparatus, because in this process the ammonia, or rather the acetate of ammonia, has less time to evaporate. That I might not be mistaken with respect to the nature of these vapours; and to collect a part of the ammonia, which is volatilized and lost in the atmosphere in proportion as the water is produced; I poured a few drops of muriatic acid into the phial, which in Meusnier's apparatus is employed to receive the liquid formed by the combustion. After having obtained 4½ oz. of this water, which was thus mixed with muriatic acid, I subjected it to spontaneous evaporation in a place where I could not suspect the presence of any ammoniacal vapours, and obtained a residuum containing 3½ grs. of muriate of ammonia, perfectly characterized by its crystallization and other properties. It was at first mixed with a small quantity of muriate of lime and muriate of lead*: the deliquescent salt was separated by elutriation; and the insoluble metallic salt by dissolving the residuum in distilled water.

With this acid it forms muriate of ammonia.

Greater part of the ammoniacal gas lost.

I could not judge by this result of the quantity of nitrogen contained in alcohol, because the vapour of muriatic acid formed a smoke of muriate of ammonia, the greater part of which escaped out of the vessel employed to receive it.

The ammonia not produced by the azote in the air.

It is not probable, that this ammonia was owing to the combination of the hidrogen of the alcohol with the nitrogen of the atmospheric air, for it has been seen, sect. II, that the latter was not condensed in the combustion of the

Lead dissolved from the worm.

* The worm of my apparatus is of lead. In this case the water produced by the combustion of the alcohol held the metal in solution probably by means of acetic acid. The water thus obtained gave a black precipitate with hidrosulphuret of potash, even when there was no muriatic acid in the receiver; but it did not produce this effect, when it was collected from alcohol burned under a glass jar.

alcoho

alcohol. Besides it will be shown by more direct observations, perfectly free from objection, that alcohol contains this element.

This result is of importance to the theory of fermentation.

Mr. Thenard had remarked*, that the nitrogen, which is an essential part of yeast, disappears in the vinous fermentation. This element was not then found among the products of this process, but we shall see, that it enters into the alcohol. Azote an essential part of yeast.

The ammonia contained in the liquid formed by the combustion of alcohol appears to me neutralized by acetic acid. I have poured a few drops of potash into two ounces of this water. The alkali, which was in excess, was saturated by carbonic acid, and slightly dried in the open air. I washed the whole with alcohol, and the decanted liquor afforded by evaporation a very deliquescent salt, which had all the other properties of acetate of potash, and weighed $1\frac{1}{4}$ grain. The ammonia neutralized by acetic acid.

All the trials I have just mentioned of the water obtained from perfect alcohol, repeated with water obtained from spirit of wine rectified without muriate of lime, gave the same results. They showed, that it contained ammonia, acetic acid, and lime, and probably a little potash: but all these substances were in such small quantity, that they could not have much influence on the proportions of oxigen, hidrogen, and carbon, assigned to alcohol by my last analysis, sect. III, where I considered the fluid formed by burning it as pure water. The water from both kinds of alcohol contained ammonia, acetic acid, lime, and probably potash, but in quantities of no importance.

Sect. V. *Analysis of alcohol by means of a redhot tube of porcelain.*

Several chemists have noticed with more or less accuracy the nature of the principal products afforded by alcohol in passing through a tube of porcelain heated red hot. They have observed water, oxicarburetted hidrogen gas, and carbon; and lastly Mr. Vauquelin mentions a crystallized volatile oil†: but they have not obtained from these products a deter- Analysis of alcohol by passing through a redhot porcelain tube.

* Essay on Vinous Fermentation by Thenard: Annales de Chimie vol. XLIX, p, 294: or our Journal, vol. VII, p. 33.

† Fourcroy's Chemistry, vol. VIII, p. 155: or English edition, p. 207.

a determination of the number and proportion of the elements of alcohol. I have attempted however, to attain a knowledge of these by the same process.

Process described.

Through a red hot tube of porcelain, glazed internally, I distilled 2183 grs. of perfect alcohol. The products passed from this tube into a glass worm* surrounded with cold water, and thence into a small globular receiver, which retained the liquid products, and allowed the gasses to pass on to the pneumatic trough.

The retort, which introduced the alcoholic vapours into the porcelain tube, was kept at a temperature between 40° and 50° R. [122° and 144° F.]. The distillation continued twenty hours. I conducted it slowly, that scarcely any of the alcohol might escape decomposition in traversing eight inches of redhot tube. From this process I obtained,

Results.

Charcoal,

1. In the porcelain tube $4\frac{1}{4}$ grs. of charcoal, which separated in the form of a thin film rolled up like a scroll, and several inches long. This charcoal, being incinerated in a platina crucible, left about a grain of ashes, in which I discovered, by lixiviation with water and solution in muriatic acid, the presence of potash, lime, and an insoluble residuum, which might be silex. Mr. Proust had already found silex and lime in alcohol.

with a little potash, lime, and perhaps silex,

Concrete essential oil,

2. The glass worm was lined with the crystallized essential oil discovered in this process by Mr. Vauquelin. These crystals presented themselves to the naked eye in the form of thin, transparent, white, and yellowish scales: but with the microscope some of them exhibited quadrilateral prisms with diedral summits. They are very soluble in alcohol; and the solution becomes milky on the addition of water, if the alcohol be not too abundant. These crystals, as well as a very thick brown oil with which they are mingled, and which is scarcely volatile at the common temperature, have a strong smell of benzoin. The weight of these two oils collected and added together, both in the worm and in the receiver, amounted to 4 grains. The receiver contained but half a grain.

and a thick brown oil, both smelling strong of benzoin.

* When I used a leaden worm, the liquor passing through it held some lead in solution.

3. I

3. I found in the receiver, beside this small quantity of oil, 196 grs of colourless water, the specific gravity of which was 0·998, indicating a mixture of 193¾ grs of water, and 2¼ grs of alcohol. These 2¼ grs therefore are to be deducted from the 2183 subjected to analysis. Water with a little alcohol,

The water I have just mentioned had a smell both of benzoin and of vinegar: it reddened sirup of violets, and infusion of litmus: it diffused ammoniacal vapours at the approach of muriatic acid: it was not precipitated by lime water, or by nitrate of mercury, but was rendered slightly turbid by nitrate of silver. This circumstance, added to the smell of benzoin, led me to suspect the presence of benzoic acid. smelling of benzoin and vinegar, reddening blue tests, and emitting ammoniacal vapours.

To find the quantities of the foreign principles contained in this water, I added it to a similar liquid obtained by the same process in another trial, and divided the mixture into three parts of 100 grs each. Analysed,

The first, evaporated to dryness in the temperature of the atmosphere, left at the bottom of the vessel a transparent varnish incapable of being weighed. it left on evaporation a slight residuum,

The second portion was mixed with crystallized carbonate of potash, which dissolved in it with effervescence. The solution, evaporated to dryness, left a residuum, on which I poured alcohol. The decanted liquor left by evaporation a white salt, which on exposure to the air speedily resolved itself into a fluid, except an infinitely small quantity of a salt in stellar crystals, resulting probably from a union of the potash with the acid that precipitated the nitrate of silver. The saline substance that deliquesced was acetate of potash. Its quantity in the dry state would have been for the 196 grs of liquid I examined 0·9 of a grain, which indicates 0·55 of a grain of glacial acetic acid in the whole aqueous product of this analysis. with carbonate of potash effervesced & formed acetate,

Lastly, the third portion was mixed with muriatic acid, to saturate the ammonia. This mixture furnished by evaporation crystals of muriate of ammonia, but the quantity was too small to be weighed. and with muriatic acid gave acetate of ammonia.

From this examination the 193¾ grs of water obtained from the decomposition of the alcohol by a red hot tube contained acetic acid in excess, ammonia, and probably benzoic Its contents.

zoic acid: but as the weight of all three together amounted to about $\frac{1}{700}$ only of the fluid that held them in solution, it may be considered as pure water, without any risk of errour, in an analysis like the present.

Oxicarburetted hidrogen gas.

4. The oxicarburetted hidrogen gas, the barometer being at 27 inches, and the thermometer at 17° R. [70¼° F.], occupied the space of 7199 cubic inches; and weighed, the day after it was collected, taking a mean between the weight of the gas that came over at the beginning, middle, and end of the process, 1786·61 grs*. Though the heat of the tube did not perceptibly vary, the gas obtained at the beginning of the experiment was lighter, and contained less carbon, than at the end. This was owing to the charcoal deposited by the alcohol accumulating gradually in the tube, and reacting on the fluid that was decomposed in proportion to this accumulation. However slowly I conducted the distillation, I could not prevent the gas from carrying over with it pretty copious white fumes, the weight of which I could not directly calculate, and the loss of which occasioned a deficiency in the results of the analysis. These fumes smelled of benzoin; and appeared to me to afford on condensation similar products with those collected in the receiver, namely, a great deal of water, and a very small quantity of oil. The latter could only be in very small proportion; for, on detonating the gas immediately after its developement, and while these fumes were suspended in it, I did not obtain more carbonic acid gas from the combustion, than when it was detonated after the fumes had been condensed in the water under the jars. The un-

Composition of the gas affected by the manner of conducting the process.

* At 28 inches of the barometer therefore, and 10° [54½°] of the thermometer, 1000 cubic inches of this gas weigh 266 grs. This result differs a little from that of Mr. Cruikshank, who makes it 237 in the same circumstances. I have performed this experiment three times, changing the diameter of the tube a little, and likewise its inclination in the furnace, and each time I found a perceptible difference in the weight of the gas and its composition. But the sum of all the products, in each of the experiments, afforded similar results for the composition of alcohol. Thus it appears, that we should be liable to considerable errour, if we did not compare together all the products of each experiment.

certainty

certainty left by the composition of this vapour however can affect only an 11th part of the alcohol subjected to analysis.

On adding together the weights of the immediate products of the whole process, we find, that 2180·5 grs of alcohol afforded

Immediate products of the decomposition of the alcohol.

Gas	1786·61 grs
Water	193·50
Oil	4
Charcoal	3·25
Ashes	1
	1988·36
Deficiency from fumes, chiefly aqueous	192·14
	2180·5

Analysis of the oxicarburetted hidrogen gas.

The 7199 cubic inches of this gas contained no carbonic acid gas. They were collected in eighteen jars, all of which were examined endiometrically. I shall give here the mean of these eighteen analyses, deducting the atmospheric air contained in the vessels previous to the distillation. With 100 parts of the oxicarburretted hidrogen gas were mixed 200 of impure oxigen gas, consisting of 190 oxigen and 10 nitrogen. The mixture inflamed by the electric spark left for a residuum some water, and a mixture of carbonic acid gas, oxigen gas, and nitrogen gas, occupying together the space of 156·5 parts. These were washed with lime water, and analysed afresh by Volta's eudiometer, adding to them hidrogen gas. I thus found, that they contained

Analysis of the oxicarburetted hidrogen gas.

Carbonic acid gas	78
Oxigen gas	65·93
Nitrogen gas	12·57
	156·5.

These

These results show, that the 124·07 parts of oxigen gas, which disappeared to effect the combustion of 100 parts of oxicarburetted hidrogen were employed to form 78 parts of carbonic acid gas, and to burn $(124{\cdot}07 - 78) \times 2 = 92{\cdot}14$ parts of hidrogen gas belonging to the oxicarburetted hidrogen gas. Thus we find, that 100 parts of the latter contain 2·57 parts of nitrogen gas. If by the rule of proportion we estimate from this the results of 7199 cubic inches of oxicarburetted hidrogen gas, weighing 1786·61 grs, we shall find, that they would have produced by their combustion 5615·2 cubic inches of carbonic acid gas, containing 945·59 grs of carbon; that the oxigen gas would have burned 6633·2 cubic inches of hidrogen gas, weighing 212·44 grs; and lastly, that the whole of the oxicarburetted hidrogen gas contains 185 cubic inches of nitrogen gas, weighing 76·77 grs.

Analysis of the oxicarburetted hidrogen gas.

If we add together the weight of the elements just calculated, we shall have, in 1786·61 grs of oxicarburetted hidrogen gas,

Carbon	945·59
Hidrogen	212·44
Nitrogen	76·77
	1234·80
Deficiency	551·81
	1786·61.

The residuum of the combustion of the oxicarburetted hidrogen gas appeared to me to be nothing but water, excepting the carbonic acid gas and nitrogen, that have been mentioned. Thus the deficiency we find on adding together the elements of this analysis must be ascribed to the elements of water, which existed in the oxicarburetted hidrogen gas not in the state of water or aqueous vapour, but in a state in which they were united and as it were confounded with the other principles of this gas. If we substitute for this deficiency therefore the elements of 551·81 grs of water, we shall find, that the 1786·61 grs of oxicarburetted hidrogen gas are composed of

Carbon

Its constituent principles.

Carbon	945·59
Oxigen	485·59
Hidrogen	278·66
Nitrogen	76·77
	1786·61*

Carbon in the alcohol.

To come at the whole of the carbon contained in the 2180·5 grs. of alcohol I decomposed, we must add to the 945·59 grs. of carbon in the inflammable gas the 3¼ grs. from the charcoal found in the porcelain tube, and that of 4 grs. of oil which might amount to about 3 grs. These added together make 951·84; and thus 100 parts of alcohol contain 43·65 of carbon.

Oxigen in the alcohol.

To find all the oxigen of the alcohol, we must add to the 285·59 grs. of oxigen belonging to the inflammable gas the oxigen of 193·5 grs. of water in the receiver adapted to the worm. Thus the sum of oxigen was equal to 485·59+170·28 =655·87 grs. From 100 parts of alcohol therefore we should have 30·12 of oxigen.

Hidrogen in the alcohol.

To obtain the whole of the hidrogen of 2180·5 grs. of alcohol, we must add to the 278·66 of hidrogen found in the oxicarburetted hidrogen gas the hidrogen of the 193·5 grs. of water collected in the receiver, and the hidrogen of 4 grs. of oil, which might be about 1 grain†. The sum of these is 302·88 grs.; so that 100 parts of alcohol would have furnished 13·89 grs. of hidrogen.

Nitrogen and ashes.

Adding to these elements the quantity of nitrogen I found in the inflammable gas, and lastly that of the ashes obtained

* This gas therefore contains in 100 parts by weight,

Carbon	52·9
Oxigen	27·2
Hidrogen	15·6
Nitrogen	4·3
	100.

† This oil does not make the five hundredth part of the weight of the alcohol I decomposed: so that in the present analysis, which is merely an approximation, I might have neglected this product; and therefore it is of little consequence, whether the composition I ascribe to it be just.

by

by the incineration of the charcoal, we find, that 100 parts of alcohol produced.

Sum of these.

Carbon	43 65
Oxigen	30·12
Hidrogen	13·89
Nitrogen	3·52
Ashes	0·04
	91 22
Loss	8·78
	100.

Deficiency to be supplied.

I noticed at the commencement of this analysis, that this loss was owing to fumes that contained a great deal of water, and an infinitely small quantity of oil, carried into the pneumatic trough by the oxicarburetted hidrogen gas. If for this loss we substitute 8·78 parts of water, we shall find, that 100 parts of alcohol contain.

Real proportions of the elements.

Carbon	43 65
Oxigen	37·85
Hidrogen	14·94
Nitrogen	3·52
Ashes	0·04
	100·

Results agree with those in Sect 2.

The results of this analysis are nearly similar to those I obtained by the detonation of the elastic vapour of alcohol in a Volta's eudiometer, section III, setting aside the nitrogen, which I could not calculate in that process, and which remained confounded with the water in the state of ammonia, if not almost wholly with the 41·36 parts of oxigen, which that analysis ascribed to the alcohol. If from these 41·36 parts of oxigen we substract the 3·52 of nitrogen we have just found, the two analyses will agree better than could have been expected from such a complex process.

Alcohol rectified alone gave similar results.

I analysed by means of a red hot tube spirit of wine rectified by simple distillation, and found no difference of importance

tance between the two results, when I deducted the quantity of water in this spirit calculated from its specific gravity.

(To be concluded in our next.)

V.

Description of an improved Mode of constructing Muffles for Chemical Purposes, by Mr. EDMUND TURRELL, *No.* 40, *Rawstorne-Street, Goswell-Street Road**.

HAVING experienced much inconvenience in the common mode of moulding muffles on wooden blocks, for the use of chemists, enamellers, &c., I beg leave to lay before your praise-worthy Society, an improved method, possessing the following advantages: namely,

Common mode of moulding muffles inconvenient.

First, By this new method of moulding muffles, coarser and cheaper materials may be used than can be employed in the common mode; and which also gives them the valuable property of resisting a greater degree of heat.

Advantages of the new mode.

Secondly, That much time will be saved by this improved method of manufacturing them, must be allowed, when the two modes are compared.

Thirdly, The certainty of making them without cracks or flaws, and with coarser materials, will appear obvious, when it is considered, that by this improved method, they are *internally* moulded instead of *externally;* by which means the strength of the operator may have its full effect, in firmly compressing the composition into the mould.

In the old mode, the workman, after having spread the composition upon a cloth, guessing at its thickness, bends it over the block in the best way he can; and by thus disturbing the composition, he must needs make many cracks and flaws, which can be but imperfectly closed in smoothing the surface of the muffle while upon the block; the evil consequence attending which is, its being subject to fly or crack

Disadvantages of the old mode.

* Transactions of the Society of Arts, for 1807, p. 38. Ten guineas were voted to Mr. Turrell for this invention.

 when

when exposed to a great heat; and it will also be plainly seen, that, in the old mode, a great disadvantage is felt by the sides of the muffle, while in its wet state, hanging from its centre, which also tends to crack it, as there can be nothing applied to assist it in this case, but by employing a greater proportion of cohesive clay in the composition, which, however, produces little if any advantage; whereas in the mode which I have invented, this fault is entirely obviated, and the composition, by its contraction in drying, assists the extrication of the muffle from the mould.

Farther advantages of the new method.

Fourthly, With respect to simplicity, this new mode will be found to possess a very great advantage, for a boy of twelve years of age may be taught to make them in a very short time.

The fifth advantage of this improvement, and of equal consideration, is the cheapness of the article; the price of which has been reduced nearly one third to the consumer; and when the superior quality of them is taken into consideration, it may fairly be said to be one half. I mean, when regard is had to their superior quality; and that the muffles may be used over again when broken and ground, with a much less proportion of cohesive clay than in the old mode; and this I conceive to be no inconsiderable advantage; for it is well known, that when the old muffles or broken crucibles can be used without much fresh clay, they are far superior to new materials.

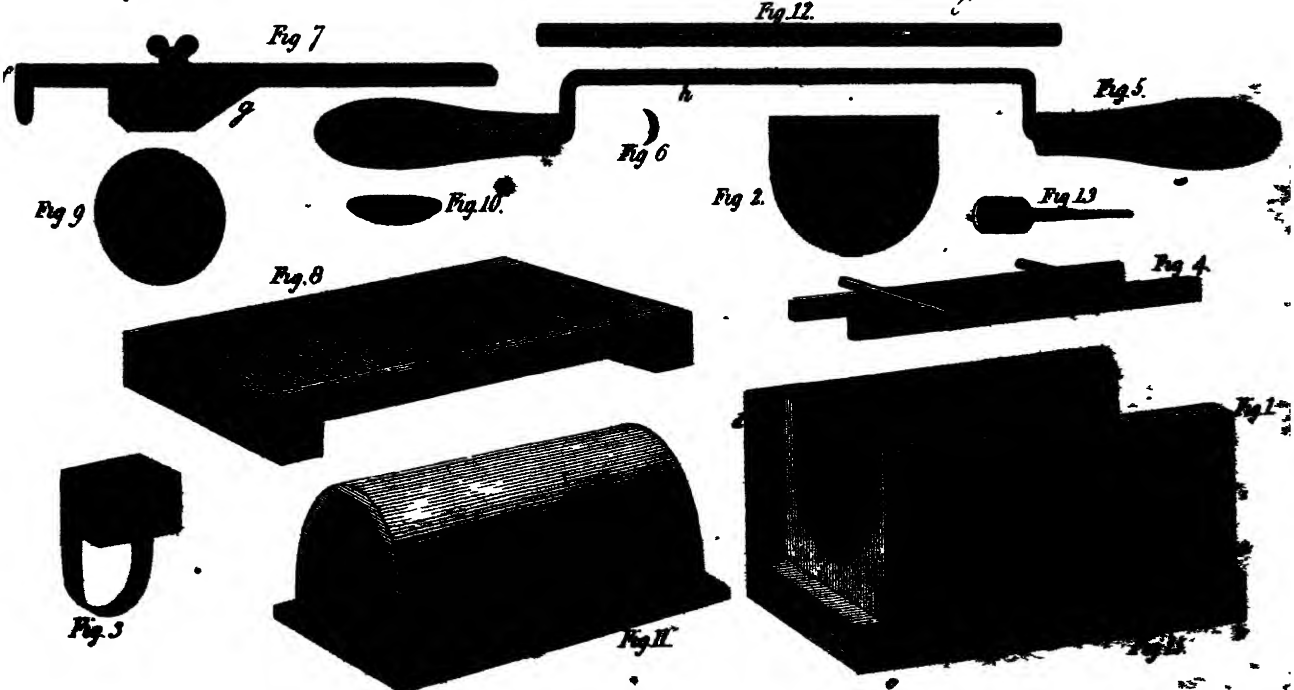
Sixthly, The muffles made in the old way are seldom of equal thickness; whereas those made according to the method which I have the honour to present before the Society, will be found to possess that necessary quality in perfection; for, if a hundred are made from the same mould, they will be all of the same thickness.

Description of the Moulds and Implements.

Description of the method of making the muffles.

The first mould for this purpose is a tin one, Plate VIII, fig. 1, which may be made from a piece of tin the size of the arch, being bent so as to form such a concavity as may best suit the purpose to which it is to be applied. This being done two square pieces of tin, *a a*, must have an arch cut out of them,

Mr E. Turrell's Construction of Chemical Muffles



them, of such a size that the diameter thereof may be about three fourths of an inch less than the diameter of the concave piece before stated; these, being soldered to each end of the first-mentioned piece, will form a stand for the hollow part of the mould, and the thickness of the muffle moulded in this will be exactly determined by the edge at each end. A piece of hollow tin, *b b*, may be soldered along the top edge of the mould, to form a better resistance to the great pressure within. The next part of this mould is a flat piece of tin, fig. 2, cut exactly to fit the inside of the mould, the use of which is, to form a solid back to the muffles used for chemical purposes.

Description of the method of making the muffles.

The second tool for this purpose is a piece of sheet brass, fig 3, about six inches long and one broad, which, being bent in a semicircular form, and screwed to a piece of wood extending beyond its breadth about an inch, is used for cutting the small air holes *c* (fig. 11), in the aforesaid muffles.

The third is the tool or frame, fig. 4, for preventing the contraction of the muffles in drying, which is made of fonr pieces of beech, about three quarters of an inch broad, and half an inch thick; the length must be adjusted to the mould of the muffle; two of these being laid parallel within the inside of the mould, and being joined across by the other two, the ends of which should extend so far beyond the outer edges of the other two, that they may rest upon the edges of the muffle mould, and thereby prevent its falling into the mould.

The fourth is the tool for spreading the composition into the moulds, which is formed of iron or steel, (fig. 5), about thirteen inches in length, one inch and a half broad, and about one eighth of an inch thick; its face under *h* being rounded in such a manner, that its curve may exactly fit the inner curve of the muffle mould, (fig. 6, is a section of it). This should likewise have a point or tongue, extending from each end, long enough to be bent in the form of a bricklayer's trowel, and by the wooden handles which must be put on, hanging down, it will be found, that, as it is moved either backwards or forwards, it will always present an edge to smooth the composition, and condense it in the mould.

The fifth is a frame (*d d*), fig. 15, of which the bottom and

Description of the method of makieg the muffles.

farthest side are only shown, and in which frame the tin mould, fig. 1, is placed, simply constructed by joining two pieces of wood, the one as broad as the bottom of the muffle mould, and having two narrow grooves (*e e*), cut in it, so that the edges of the tin mould may be confined therein. The other board, being joined to this at its edge, should come up so high as just to be under the edge of the mould.

The sixth is the tool for cutting the muffles of different lengths (fig. 7), and is made of a piece of wood, to the end of which is fixed a thin piece of brass (*f*), which, extending about one inch and a quarter beyond the top of the wood, is bent at right angles, and made thinner at the end, that it may the more conveniently cut the muffle. Under this piece of wood is used another straight piece (*g*), with two steady pins, which, being shifted at the will of the workman, will cut them of any length.

The seventh ia the mould for forming the bottom of the close muffle (fig. 8), which is made of a mahogany or oak plank, about sixteen inches long, ten wide, and about three eighths of an inch thick; upon this is fixed a ledge on each side, one inch broad, and nearly half an inch thick, and at each end a ledge of the same kind is placed, at such a distance as is best suited to the length of the bottom required. Fig. 9 and 10, are circular moulds for muffle bottoms of dial plates. Fig. 11, a complete muffle standing on its bottom. Fig. 12, a roller for rolling the composition in the first mould. Fig. 13, a tool for making small holes in the muffle.

The usual composition for making muffles is as follows: viz. two parts pipe clay and one part sand, such as is used by the bricklayers, sifted, and mixed together to a proper consistence; this is very expensive, on account of the high price of pipe clay, which is about ten shillings the hundred weight, whereas I employ in my improved mode of making them the coarser kind of Stourbridge clay, which can be had at the glass-houses, in the ground state, for six shillings the hundred weight; and this I sift also, to separate the finer part, which I employ for making other smaller articles necessary in my business; using only the grosser or coarser part for muffles, to which I add one eighth part only of pipe clay, mixing them

well

well together with water, so as to form a mass of a pretty thick consistence. The tin mould being first greased, I place it in the frame, fig. 15, shown under fig. 1, and having spread the composition in the mould, and smoothed it with the spreader, fig. 5, till the mould is quite full, the flat piece of tin is then to be well greased, and thrust in at one end of the mould, and the back of the muffle is then formed by spreading the composition, and firmly pressing it against the part already formed. The next thing to be done is to cut the holes in the sides of the muffle, which is done by pressing the semicircular cutter, fig. 3, into the sides thereof, while it is yet wet, and bringing the piece out entire: the tin mould must now have the frame, fig. 4, put on, to keep the sides of the muffle from contracting; and being set up endwise, and a little inclined, it must be dried in the sun, until it has shrunk sufficiently to leave the mould, after which it must be completely dried and burned in the usual manner.

Description of the method of making the muffles.

The composition of the smaller implements, or muffle bottoms for dial plates, for the mould figs. 9 and 10, is made of the finer part of the Stourbridge clay, with a small proportion of pipe clay.

The rings are made from two parts of Dutch black lead pots powdered, and one part of pipe clay. I have made repeated trials of English black lead, in various states, as a substitute for the Dutch black lead pots, but without finding it to answer properly.

Should any difficulty appear in any part of my process, I shall be happy in attending the committees, and performing the whole operation before them, whenever they shall be pleased to appoint, when the great simplicity and advantage will appear evident.

I am, my Lords and Gentlemen,

Your most obedient and respectful Servant,

EDMUND TURRELL.

No. 40, *Rawstorne-street, Goswell Road,*

April 10*th*, 1806.

To the MEMBERS of the SOCIETY of ARTS, &c.

Certi-

Certificates from Messrs. J. Haynes and Son, Westmoreland Buildings; John Kelly, Hooper-Street, Clerkenwell; John Foster, Author Street, St. Luke's; and William Foster, Author Street, St. Luke's, states, that they have been in the habit of using Mr. Turrell's muffles for upwards of twelve months, that they are greatly superior to any they have hitherto been able to procure; and that it is their opinion their durability may be completely attributed to his improved method of moulding them.

VI.

Considerations on the State in which a Stratum of nonconducting Matter must be, when interposed between Two Surfaces endued with opposite Electricities: by A. Avogadro, *Corresponding Member of the Academy of Sciences at Turin**.

SECTION I.

State of a nonconductor between two conductors not sufficiently examined.

THOSE learned natural philosophers, who have lately studied with so much success the mechanism of the forces, that electricities of the same or opposite kinds exert on each other, either in conducting substances, or through nonconductors, have not paid equal attention to the facts, that may lead us to some knowledge of the state of the insulating substance† through which these forces act; particularly when it is interposed between two electricities of opposite kinds, that mutually support each other by their attraction. Yet if such facts exist, they might lead us to consequences of great importance to the theory of electricity. In reality the circumstances I have just mentioned, the interposition of an insulating stratum between two bodies endued with opposite electricities, is of great extent in electrical phenomena: it not only takes place with respect to substances made to approach each other expressly for this purpose, and in charg-

* Journal de Phisique, vol. LXIII, p. 450.

† In this paper I employ the term *insulating* as synonimous with what is commonly called *nonconducting*, or *electric per se*.

ing

ing the Leyden phial, or a plate of a glass, which in fact is nothing more than a method of bringing the two coatings nearer together than could be done in the air: but it introduces itself generally into all the electricity of conducting substances, as is easy to be observed; for every electrified body is surrounded with other bodies more or less distant, on the surface of which, according to the established principles, the electricity of the former body can only occasion an opposite electricity by acting through the intervening stratum of air. We may truly say, therefore, that there is no electricity but has opposite to it the contrary electricity, with an intermediate insulating stratum.

Leyden phial.

A general case.

On the other hand, were we once convinced of the insulating stratum in these circumstances being in a peculiar state, distinguishing it from a simple medium through which the electric forces exert themselves, it is natural to suppose, that a more attentive examination of this state would give us some idea of the manner in which those forces exert themselves, which the labours of philosophers have hitherto only confirmed; but which, according to all appearance, are not to be ascribed to an original property inherent in the substances that exert them, as has been asserted with great probability in respect to the Newtonian attraction of matter in general.

Electricity not an inherent quality like gravitation.

Now reflecting on certain facts, that Symmer, Cigna, Beccaria, Volta, and others have established by their experiments, it has seemed to me, that we might deduce from them some inferences relative to the state of the insulating stratum in question. The object of the present paper is to detail the ideas, which these facts have suggested to me.

Facts leading to a knowledge of the state of the nonconductor.

Sect. II. The fundamental experiment*, which first presents

Fundamental

* The facts adduced in this and the following section are not new; neither indeed are the reflections accompanying them wholly my own. Æpinus, Haüy, Volta, and others, have already given them at least in part, and in a form more or less resembling that in which I exhibit them; but perhaps they have not paid them sufficient attention in general. I have thought it necessary, to resume this subject in a somewhat more extended way, as an introduction to the ideas that constitute the principal object of this memoir, and which I begin to lay before the reader in section IV.

experiment when two non-conductors charged with opposite electricities come together, the electricities disappear.

presents itself in this branch of electrical science, is the following. When two insulating bodies, or one insulating and one conducting body, the surfaces of which are endued with opposite electricities, come to be applied to each other by their surfaces, the electricity of both seems to disappear. We no longer find any signs of it, or at least if any sign of either kind of electricity remain, they may easily be deprived of this surplus: but then if we endeavour to separate these two bodies, we find, that they adhere together, which proves, that all their electricity was not really destroyed; and if we overcome this resistance, and actually separate them, we shall find, that each of thess bodies again exhibits signs of that kind of electricity, which it possessed before they were brought together. These phenomena however are not to be observed in all their simplicity, except with insulating bodies of a texture sufficiently thin to be incapable of an electric charge; and such as have a kind of communication between their surfaces, as with two ribands for instance, or two silk stockings, or one of these and a piece of insulated tin foil. I shall not here enter into the particulars of these experiments, which may be seen in Priestley's History of Electricity, Mr. Symmer's communications to the Royal Society, Mr. Cigna's paper in the 3d vol. of Miscellanies of the Royal Society of Turin, &c.

But it is not destroyed.

Only a particular case of an acknowledged general law.

Before I introduce this fact into the examination, that constitutes the principal object of this paper, it may not be amiss to show, that it is merely a consequence of the known principles of electricity, a particular case ot a law, the generality of which is at present acknowledged.

The thinner the interposed substance, the stronger the opposite electricities.

It is known, that one kind of electricity is capable of being so much the more condensed on a surface in proportion to the proximity of another surface endued with the contrary electricity, the attraction between the two kinds of electricity in this case surmounting with more advantage the repulsive power, which opposes this condensation in each kind of electricity. It is known likewise, that for this reason the coatings of a Leyden phial can acquire much greater quantities of electricity than bodies of equal surfaces electrified in the air; and it is a practical truth long known, and depending on the same principle, that the

Leyden phial.

phials.

phials, panes of glass, &c., have so much the greater capacity for a charge, other circumstances being equal, in proportion as their thickness is less.

Case of two electrified ribands.

This admitted, let us suppose, that the surfaces of our two ribands, endued with opposite electricities, are gradually brought nearer to each other, keeping them parallel. The repulsive force of each of these two electricities will render itself so much the more perceptible, and t ey will have so much the less tendency to be conveyed away by the surrounding bodies, as their distance is diminished; because the attraction between the two electricities will become so much the greater: and when at length the surfaces are brought into contact, the attraction having become as it were infinite, these electricities will no longer tend to fly off, but will remain as if they did not exist with respect to other bodies, whatever intensity they had before, since this intensity was limited, and the repulsion arising from it was also limited.

Charged plate of glass.

This may be exhibited in another point of view. When we charge a plate of glass, the electricity produced on the interior face of the coating opposite that which is electrified directly has no tendency to fly off, because it is perfectly retained by the attraction of the electricity of the latter coating; an electricity which, according to the principles of Coulomb and Haüy, to produce this effect must be conceived somewhat greater, than that which is produced on the opposite face of the plate. The electricity of the face electrified directly is on the other hand perfectly retained by that of the opposite face, with respect to the portion equal to it: and it is only its excess that has a tendency to be dissipated, and requires the resistance of the air to retain it. Now Haüy has already observed, that this excess, according to the theory, must be so much the less, in proportion to the thinness of the plate; and that it would be nothing, if the plate were infinitely thin, or in other words nought. Neither of the two electricities then, that compose the charge, would any longer tend to fly off; they would become insensible. This is precisely the case of the electricities of our two ribands, considered as coatings of the stratum of air at

first

first interposed between them, and which becomes nought by their contact.

The two electricities not destroyed by the contact.

There is nothing here at which we need be surprised, except, that the two electricities are not destroyed by the contact, where nothing appears to prevent their mutual attraction from exerting itself. But we may suppose, that this attraction is sufficiently satisfied by the mere contact, that this contact serves instead of actual communication, and that it neutralizes the two electricities, as communication itself would do; for it is sufficiently proved, that this does not take place, and that the two electricities still belong to the two faces separately, since they immediately manifest themselves when the two surfaces are again separated so that a fresh stratum of air is introduced between them. The resistance we experience in this act of separation is likewise an effect of the two subsisting electricities, the mutual attraction of which necessarily opposes a separation of the surfaces, which carries with it that of the electricities.

Why the phenomena cannot take place between two conductors.

It is easy to perceive however why we cannot observe these phenomena between two conducting bodies; for as the contact of the two surfaces can never take place accurately and and instantaneously at every point, the first point of contact between two bodies of this kind is sufficient to destroy the whole electricity of the two surfaces, which still retains its intensity, and is not yet neutralized by contact. The same thing would take place on the separation of these bodies, even if we supposed them possessing this electricity, though imperceptible, in the state of contact. The retention and apparent reproduction of the electricities therefore cannot take place, unless one of the two bodies at least is an insulator.

As the electricity was supposed to be destroyed, nonconductors were supposed to reproduce it on separation.

Before I proceed farther I shall observe, that the total loss of intensity, which the electricities experience in the contact in question, has led some to imagine, that the electricities really destroyed each other by communication: particularly as they could not conceive what should prevent this communication from taking place: and in consequence they were obliged to suppose, that insulating bodies had the singular property of resuming on separation the electricity they had deposited on coming into contact; that they *reclaimed* it

it as it were: and hence the name of *claiming* electricity (*electricitas vindex*), which Beccaria gave the electricity thus apparently reproduced. The adhesion of the two surfaces on their contact however had led others to presume, that the electricity was merely latent, and not annihilated.

This question, which presented no clew to its elucidation, and which was reduced almost to a dispute about words, or to a different mode of viewing the same object, while the phenomenon in consideration was examined separately, solves itself now we perceive its connection with known principles: for it is demonstrated *a priori*, that the electricity in this case must lose its intensity, and become imperceptible, though it is not really destroyed: We may express this state by the term of *quiescent electricity*, or electricity at rest.

The electricity is in fact only quiescent.

SECT. III. Let us now pursue the inquiry. I have said, that it is only with insulating substances of a thin texture we can observe the phenomena of which I have spoken in all their simplicity. It is easy to conceive with respect to compact bodies capable of being carged with electricity, as for instance two plates of glass, that the dependance, which the electricity of one of the faces has or may have on the electricity of the face opposite to it, with which it forms or may form the electric charge of the plate, must necessarily render more complex the phenomena relative to the state of rest, and to the revivification of the electricity of the faces, that are brought into contact or separated. I shall not enter here into the details the subject would require. Though several natural philosophers have already engaged in researches of this kind, much remains to be done, to illustrate it completely*. Nothing more, is necessary for my purpose here,

When the intervening non-conductor is thick the phenomena appear more complex.

*The first experiments on quiescent electricity, and its revivification in compact bodies, were made by the Jesuits of Pekin, and communicated to the Academy of Petersburg in 1755. These gave occasion to a paper by Æpinus in the 7th vol. of the New Transactions of that Academy. Symmer treated the same subject in his fourth paper, read to the Royal Society in 1759. Lastly Beccaria entered into it very largely in his book entitled "Observationes atque Experimenta, quibus Electricitas vindex late constituitur, and explicatur. Turin, 1769. The reader may likewise

History of the experiments on this subject.

two plates of glass discharged as a single one.

here, than to relate the simplest case. Let us suppose, that, after having charged two plates of glass, those faces are brought into contact, which are charged with opposite electricities, being previously divested of their coating: and a communication between their exterior coatings is then established; in other words, that the two plates thus joined are discharged as if they were a single plate. The plates thus joined will no longer give any sign of electricity, and the two exterior electricities are destroyed by this communication, as if there were no others. As to the interior electricities, it seems at the first view, that they must have been annihilated at the same time by their mutual communication, the dependence of each on one of the electricities of the exterior faces having ceased. But this is not the case: these two electricities being in contact must merely neutralise each other, according to the principles of quiescent electricities, by this contact, as soon as the anterior electricities, having destroyed each other, cease to maintain them separately, They become imperceptible in consequence of this neutralization only, and ought consequently to oppose each other like those of the ribands mentioned above, when we separate the plates again. And this is what experience in fact demonstrates: for, if we attempt to separate the two plates after having discharged them together, we find a resistence as much superior to that displayed by insulating bodies of a thin texture under similar circumstances, as the electricities that concurred to form the charge of the two plates, and which are here converted into quiescent electricities to be revivified by separation, are superior to those that could be imparted to the bodies of a thin texture. And if we overcome this resistance, and actually separate the two plates, the two electricities of the interior faces will resume their intensity, and their tendency to decompose the natural electric state of the surrounding bodies, and in particular of the interior face of the coatings with which the exterior surfaces of the two plates are covered: whence it follows from the known principles, that the exterior faces of these

see what he says on the subject in his *Electricisme artifiziale*. The theory Volta has given of his electrophorus and condenser likewise regards the same subject. I shall have occasion to notice these hereafter.

coatings

coatings will give signs of an electricity of the same kind as that of the interior face corresponding to each plate.

The phenomena exhibited by a compact, charged, insulating plate, one of the uncoated faces of which is brought into contact with a conducting body, depend on the same principles; but they would also require a minute detail to be treated fully. To this class of phenomena belong the well known effects of Volta's electrophorus: and the same gentleman has freed them to a certain degree from the complication respecting the charge of the insulating body, in his semiconducting plate doubler, the effect of which appears to me, to belong essentially to the phenomenon of quiescent and revivified electricity, exhibited between a perfectly conducting substance, and a substance of sufficient conducting power to exhibit this phenomenon in its simplicity, as incapable of being charged, and yet so bad a conductor as to afford a charge; while, as we have seen, it cannot take place between two perfectly conducting bodies. But I cannot here enter into the particulars, on which the theory of these two instruments depend. I shall only say, that what Volta himself, and since Haüy, have said of it, appears to me essentially to require the principles in question; but as this theory could not be completely developed by these gentlemen, because they had no farther object than to explain the effects of these instruments, they have not generalized it sufficiently. In the course of this paper however, I shall have occasion to touch on some points relating to this subject.

One of the uncoated faces of an electrified nonconductor brought into contact with a conductor.

The electrophorus, and doubler.

Sect. IV. Let us now return to the point in question, and apply what has been said of the particular case of the two plates, at which we had stopped, to the inquiry we had in view respecting the state of a charged insulating stratum, or that interposed between the opposite electricities.

Charged nonconductor.

For this we want only one more fact, which is equally well established. It is that if, after having charged the pair of plates joined together by their uncoated faces, as if they were a single one, they be discharged in the usual way, and we afterward endeavour to separate the two plates, we observe the phenomena of the revivification of the electricities, similar to those obtained from two plates charged separately, afterward joined

Two plates coated on one side, and charged as a single one, exhibit the same appearances, as when charged separately.

joined together by those faces that have opposite electricities, and discharged in this state as we have already said. This proves, that in the combination of two insulating plates, thus forming but one body, each of the plates takes its own charge; that is to say, there is formed on the lower face of the upper plate an electricity opposite to that communicated to its upper face; that in like manner an electricity is formed on the upper face of the lower plate of the same kind as that communicated to the upper plate; and lastly on the lower surface of the lower plate an electricity opposite to this: and thus the lastmentioned electricity does not correspond directly to the opposite electricity of the upper face of the upper plate, but depends on it only through the medium of the intervening electricities of the two interior faces that are in contact. In fact, since the two plates when separated after their discharge exhibit the same electricities, whether they be charged together or separately, they must be in the same state after the discharge in both cases: but this supposes likewise the same modification in the charged state, since the discharge is made precisely in the same manner, and with the same phenomena, in both cases. It is unquestionably the same, when more than two plates are thus combined; each of them must undergo the same modification as if it had been charged separately, for the number makes no difference here.

The same with any number of plates.

One solid plate therefore may be considered as a number of infinitely thin ones.

Now as any compact plate may be conceived to be divided into as many strata as there are elementary molecules in its thickness, all these strata must be considered, when the plate is charged, as having each its particular charge, so that the face of one, which is charged with either kind of electricity, is successively in contact with that of another, which is charged with the opposite electricity: for as to the effect in question it can make no difference, whether the strata be simply in contact or adhere together, since in both cases they form but one continuous substance.

This the general principle.

The following is the idea therefore that facts have led us to form of every insulating stratum charged with electricity, or, which comes to the same thing, taken between two opposite electricities: It ought to be conceived of as formed of an infinite number of strata, all which, however thin they are,

are, exhibit on their opposite surfaces electricities of opposite kinds, as well as the assemblage resulting from them.

Coulomb and Haüy have been led to an analogous result in their inquiries concerning magnetism, and the electricity of the tourmaline, but they had not extended this idea to every charged insulating stratum. It appears however, that the modification of the heated tourmaline is not even a particular case of the general principle we have just admitted: that this stone is not then simply a charged insulating body, or a body interposed between two contrary electricities; but exhibits on its surface a modification of electricity, which is perhaps more analogous to the state of a conducting body, the natural electricity of which is decomposed. But this is foreign to our purpose.

Coulomb and Haüy infer the same if magnetism and the electricity of the tourmaline.

The addition that has just been made to our ideas respecting electricity obliges us to a small modification of our nomenclature likewise. Hitherto we have considered an electricity, which is on the surface that serves to limit two different bodies, as belonging to the surface of either indiscriminately. Thus the electricity that is between the interior surface of a coating, and the surface of a plate of glass to which the coating is applied, might equally be called the electricity of the coating, or the electricity of the face of the plate. Yet this electricity may have a different relation to these two surfaces: one may be that of a body, through which the electricity in question supports itself by its attraction for another electricity of the opposite kind, and in the thickness of which it consequently occasions the peculiar modification we have established: while the other of these surfaces may be nothing but the mechanical support as it were of the same electricity, or belong to a body, through which it does not exert the particular action abovementioned. This is precisely the difference between the surface of a plate of glass and that of its coating, or more generally between the surface of the air that surrounds an electrified conducting body and the surface of that body: for we see clearly, that a conducting body cannot have two electricities of a different kind on its surface separated by its thickness alone. It is improper therefore, to give the same name to these two different conditions. To distinguish them without deviating

This requires some alteration of terms.

more

more than is necessary from established custom, I shall say, that a body is *electrified*, or *endued* or *animated* with electricity, when the electricity remains on this surface without occasioning in the body to which it belongs the modification I have spoken of, whether the body be a conductor or an insulator; and with respect to the surface of the body that undergoes this modification, I shall merely say, that the electricity is applied to it, which can only take place for an insulating body. Thus in a charged plate of glass it is the coatings that I consider as *electrified;* but the electricity of the interior face of each coating is *applied* to the face it coats. It may happen, that an electricity at the limits of two bodies may affect these bodies equally with the modification in question; and then this electricity may be considered at once with respect to each of these bodies as belonging to their contiguous faces, or applied to them.

Charged plate

The name of electric charge will continue to indicate, as it has hitherto done, the state of an insulating stratum interposed between two electricities of opposite kinds, namely, that to the opposite faces of which these electricities are applied, a state which is the subject of the present paper.

What takes place in the discharge.

Sect. V. The facts, that have led us to form an idea of the electrical charge, necessarily give us likewise more accurate ideas of what passes in discharging a charged insulating body. It is clear from what we have seen to take place, in the two plates of glass united, that the discharge only obliges the opposite electricities, which supported each other alternately through each of the strata into which the insulating body might be conceived to be divided, to become *the electricities of the faces of these strata,* to which they were respectively applied before the discharge, and to rest against each other in pairs in a state of perfect repose; namely, that of each face against that of the contiguous face of the next stratum; so that, instead of an infinite number of charged but very thin strata, the result is an infinite number of pairs of electricities neutralized by contact.

The extreme electricities do not destroy each other by the communication.

What has been said in speaking of two plates might lead us to suppose, that this transformation, this different arrangement of electricities by pairs, was the consequence of the extreme electricities of the whole stratum, that is to say, the two

two electricities of the interior faces of its coatings, mutually destroying each other by communication: but this is not quite accurate. In fact experience teaches us, that there is a revivification of electricity even between one of the faces of a plate and its coating, when we come to take it off after having successively charged and discharged it. Even the fundamental property of the electrophorus of Volta is connected with this phenomenon, if we consider the electricity imprinted on the plate as occasioning a charge there, which is true at least with respect to one part of this electricity; and the disk as a coating, by means of which we destroy this charge in touching it before lifting it up. By this separation the coating or disk is made to exhibit an electricity of the opposite kind to that which it had when it was in contact with the charged insulating plate; and on the contrary the face of the plate exhibits the same kind of electricity, as the coating had before the discharge, and which then consequently was only *applied* to this face. It is clear from this, that the discharge has not actually taken away the electricity, that was applied to this face; and occasioned the charge of the plate; and that it has done nothing more than oblige it, from electricity of the coating, which it was before, to become electricity of the face of the insulating body, and in this quality rest itself against another contrary electricity, which was formed by the discharge of the interior surface of the coating: in the same manner as that of the other strata, which we conceive in the insulating body, rests after the discharge on the electricity that was applied to the face of the contiguous stratum, and becomes the electricity of this face.

Electrophorus.

It is the same with the opposite coating. It is then by the mutual decomposition of the natural electrical state, to which the two coatings are reduced by the transportation of their preceding electricities to the faces of the insulating plate, that the discharge is made: to form these two new electricities, which become quiescent, and are necessary to place in the same state all the other electricities of the insulating stratum; these two electricities of opposite kinds, which each of the two coatings acquires at the expense of the natural state of the other; is the end of that transportation of the fluid, which occasions the shock; and not to destroy

In the discharge the electricity from each side is transferred in part to the other, and thus renders it quiescent:

destroy the existing electricities by a communication between them.

and thus the number of electrical srata are increased by two.

Thus, if the preceding considerations have already shown us, that the discharge does not really destroy all electricity in insulating bodies once charged, but merely changes perceptible electricities into quiescent, a result that may appear singular, the last reflections that have been made led us to a result still less expected, namely, that the discharge increases the number of these electricities by two, to render them all imperceptible.

The mode in which electricity acts may be the subject of future inquiry.

SECT. VI. It remains now to inquire, how far these new ideas of electrical charges and discharges, or of the modification assumed by an insulating stratum interposed between two electricities of opposite kinds, may facilitate our investigation of the mode in which electricity acts: but this inquiry, which demands farther preliminary reflections on other points of electrical science, may form the subject of future communications.

VII.

Account of an Experiment in which Potash calcined with Charcoal took Fire on the Addition of Water; and Ammoniacal Gas was produced. In a Letter from JAMES WOODHOUSE, *University of Philadelphia, &c.*

To the EDITOR of the PHILOSOPHICAL JOURNAL.

SIR, *Philadelphia, Sept.* 15th, 1808.

Soot and pearl-ash exposed to an intense heat.

HAVING been engaged in the analysis of soot, I exposed half a pound of this substance in powder, mixed with two ounces of pearlash, in a covered crucible, to the intense heat of an air furnace, for two hours.

When cold took fire on the addition of water.

Not hidrogen but ammonia.

When the mixture became cold, it was emptied upon a plate, and a small quantity of cold water poured upon it, when it immediately caught fire. Expecting there was a decomposition of water, I placed my nose over the mixture, in order to smell the hidrogen gas, which I supposed

posed would be thrown off, but was astonished to find a disengagement of ammoniacal gas. cal gas evolved.

The experiment was repeated with common charcoal, with exactly the same result. Charcoal gave the same result.

Azote is one of the component parts of ammonia. Now, as this base is not contained in either potash, water, or charcoal, whence did it arise, to form the ammoniacal gas? Whence came the nitrogen?

Is it one of the component parts of potash? or is this substance a triple compound, formed of oxigen, azote, and the peculiar metal, which Professor Davy has discovered? Is it a componen part of potash?

Nascent hidrogen sometimes combines with the azotic portion of atmospheric air, and forms ammoniacal gas; but this is not the case in my experiment, for, if the fire of the mixture of charcoal and potash be extinguished by water, and it is then immediately placed under a bell glass containing atmospheric air, the oxigenous part will be absorbed; and the azotic air, will be left behind. The ammonia not from nascent hidrogen combining with the nitrogen of the atmosphere.

No carbonic acid will be formed.

I am, Sir,

Your humble servant,

JAMES WOODHOUSE.

VIII.

On the Advantages of employing Coal Gas for Lighting small Manufactories, and other Purposes. In a Letter from Mr. B. COOK.

To Mr. NICHOLSON,

SIR,

I Have taken the liberty, from reading in your Journal for September, the paper of Mr. Murdock on the gas light, to address the few following remarks to you. The more the advantage arising from the use of gas is clearly stated, the more generally and simply it is explained, to induce manufacturers and others to make use of it, I think the better; especially now, through the present rupture with Russia and the other It would be beneficial to state the advantage of gas light clearly.

northern powers, the want of importation of tallow has increased to a very considerable height the price of candles, soap, &c. The rise of price in candles has of course been the occasion of an equal rise in oil, as lamps are substituted in the place of candles.

Coal abundant in this country:

This country produces a vast quantity of coal, in almost every part where it is properly sought for, and if the gas light was generally introduced into the greatest part of the large, the middling, and even the smaller sized manufactories, a natural consequence would be, that coal would be consumed in much greater quantities. It might raise the price, but it certainly would be a stimulus to men of landed property, to seek for it, where to the present it has been supposed a stranger. It would therefore, if the demand was so much the greater, be found I am sure in greater quantities than at the present, as miners would be induced to seek it every where. In times like the present, when we are in great measure hindered from exporting it, it would be an advantage if we could consume it all; and in fact at all times, if the whole of the coal produced in this country could be consumed, it would supersede this anxiety for exportation, especially if it brought a little higher price.

and if the consumption raised the price a little, it would occasion a greater supply.

Coak might be more generally used than at present,

In your remarks on the paper, you seem to think, that, was the gas used generally in lighting the streets, and add to it, if generally used in manufactories also, the great quantities of coak produced would be so much more than the demand for it, that it would sell much lower than the present market price. This would certainly be the case, unless coak could be introduced into more general use than it is at present. But from experience I find, that a fire made of coak will last much longer than one made of coal; for, the gas being extracted, it loses that degree of inflammation, which, at the time it blazes, consumes the coal very fast; especially if it is good coal, which contains a large quantity of gas. When I speak of coak used in the place of coal as an advantage, it is in stoves in warehouses and shops, where stoves are in more general use than fire-places. These having a quick draught, the coal, especially as I said before, if good, soon flares away; but if coak is used, that inflammatory principle being taken away, it glows, casts out a great

at least in stoves in shops and warerooms

a great heat, and lasts much longer. I may say two fires of coak will last longer than three made of coal, so that I do think that coak, bought at its present price, is equally as cheap, if not cheaper, than coal. Therefore manufactories would experience no difficulty from the increase of coak, as each manufacturer would burn his own coak: it would be only the coak produced from lighting the streets, that would be required to be sent to market, and that only in the winter quarter; and if no coak was made at the coak works, and in fact, the demand on those works would in a great measure be at an end, they would be forced to bring their coals to market.

Coak burns much longer than Coal.

It would put stop to coak works and hence an additional supply of coal.

I do not think then the coak made would be so much above the demand, for it is only in large manufacturing towns, where coaks are used in quantities; and in those towns, if they use the gas, they will make perhaps as much coak as they may want on the spot; they will therefore save all the expense of the carriage of the coak from the works where it is made. Besides, were they to sell it, they could afford it much lower on that account; for, when the coak is made at the works, the gas is all lost, beside the expense attending the making, and the carriage of it to market. It might therefore, if the streets were to be lighted by it, be afforded at a lower price, if it was found that more was made than could be used in the regular way, to people who would burn it in their stoves. It would certainly make a reduction in the profits calculated to arise by lighting the streets, if it was sold at a lower price; but this I do not think would be the case, as the demand, especially in times of good trade, is always great.

The coak would not much exceed the demand,

and therefore the price would not be much lowered.

From the tar I conceive a spirit might be made, as a substitute for the tar spirit brought from Russia, &c., that would be of vast importance to a great number of manufacturers, especially japanners, &c., that article having advanced from perhaps three shillings or three shillings and sixpence to twenty shillings per gallon since the stoppage of trade from the north. If this end could be attained, the tar would always be a source of considerable profit, and make us independant on any other country for a supply of that article.

Tar spirit might be manufactured, instead of that imported.

The

Advantages of the more general use of gas.

The general use of gas would give us several great advantages: first it would prevent the great demand for tallow; and candles would never be so expensive as at the present: secondly it would in part take the place of oil; and thirdly it would render soap lower, as the fat used for candles, might be employed for soap making, saying nothing about the possibility of making spirits from the tar. Besides, if this could not be done, the tar is a very excellent coating for all out-door work, such as gates, fencing, and paling; as well as for boat builders and shipwrights, it being a certain preservative from the worm or the rot in wood exposed to the air, or lying in the water; by coating the articles once in two years well with it. It is infinitely better than the common paint used now: besides if the thing was general, and such quantities of tar were made as would be the case, I should suppose government would recommend its use in the dock yards in order to encourage its consumption in preference to that imported; for it is without doubt superior both as a preventive of decay, and a preservative from the worm in ships' bottoms.

Use of coal tar.

All these advantages we have within ourselves, in that article which abounds in such plenty all over this island—coal.

Utility of the coal gas to small manufacturers.

I now proceed to state the benefit I have derived from the use of the gas in my small manufactory, in order that small tradesmen may make a comparison themselves, and see what an advantage they may derive from its use. Mr. Murdock's paper is on the great scale, therefore far above the calculation of the simple mechanic; and it is to the great number of these that the thing ought to be made clear. To them a small saving of ten pounds per annum is of as great consequence as to the wealthy their thousands.

Apparatus on a small scale described.

My apparatus is simply a small cast iron pot of about eight gallons, with a cast iron cover, which I lute to it with sand. Into this pot I put my coal. I pass the gas through water into the gasometer, or reservoir, which holds about four hundred gallons; and by means of old gun barrels convey it all round my shops. Now from twenty or twenty-five pounds of coal I make perhaps six hundred gallons of gas; for when my reservoir is full, we are forced to burn away the overplus in waste, unless we have work to use it as it is made.

But

But in general we go on making and using it, so that I cannot tell to fifty or one hundred gallons: and in fact a great deal depends on the coal, some coals making much more than others. These twenty-five pounds of coal put into the retort, and say twenty-five pounds more to heat the retort, which is more than it does take one time with another, but I am willing to say the utmost, are worth fourpence per day. From this fourpence we burn eighteen or twenty lights during the winter season. The candles we used were six to the pound, which on an average one time with another would be about twopence each, though now nearly twopence halfpenny. Say eighteen candles at twopence each are three shillings a day, or eighteen shillings a week; and that each man burnt his candle for twenty weeks only in the year, though for the winter quarter he in general has burnt two instead of one; making the annual amount eighteen pounds. Calculation of the profit.

Besides, my yearly expense in oil and cotton for soldering was full £30, which is entirely saved, as I now do all my soldering by the gas flame only. My trade is that of a manufacturer of toys, in metal and gold. Now in all button soldering, all the plated articles, in fact all trades in which the blowpipe is used with oil and cotton, the gas flame will be found much superior, both as to quickness, and neatness in the work; for the flame is sharper, and is constantly ready for use, while with oil and cotton the workman is always forced to wait for his lamp getting up; that is, until it is sufficiently on fire to do his work. Thus a great quantity of oil is always burnt away useless; but with the gas, the moment the stop cock is turned, the lamp is ready, and not a moment is lost. Utility of the gas for soldering.

You see my weekly expenditure in coal does not exceed two shillings; and if I allow five shillings a week to a man, to employ part of his time to attend and make the gas, the expense will then be seven shillings. The yearly expense, if I take it at the same the whole year (although for twenty-five or thirty weeks in the year none will be required as candles) will only amount to £18 4*s*. I have, I know, in the instance of candles, much underrated the expense, as also in oil. I have also estimated the expense in coal, &c., quite high enough; and the coak I find equally as cheap to burn Expenditure for gas.

burn in my small stoves in the shops as coal; so that I do not overrate this, when I say £2 10s. for it. The expense of putting up my apparatus was about £50; but not knowing the cheapest and readiest methods to go about it, it cost me more than it ought by £15. I will say £40, for which in my statement at the conclusion I shall allow interest.

Advantageous on a still smaller scale.

If erected on a smaller scale, the saving to the manufacturer is equally as great; for the poor man, who lights only six candles, or uses one lamp, if the apparatus is put up in the cheapest way, will find it cost him only £10 or £12; which he will nearly if not quite save the first year. And if the pipes are made of old gun barrels, as mine are, and once a year, or once in two years, are coated over with the tar to keep them from rusting, they will last half a century. The burst or waste barrels, that used to be sold for old iron, would then produce a better price to the gun makers; and the pipes would be better and more durable, than if made of more slight materials.

The profit underrated.

You see I have reckoned the five shillings per week for the man the whole year, as also the same expense for coal for the whole year, of course that is reckoning more than I ought by nearly a fourth; but where soldering with the blowpipe is necessary, gas will be wanting, although in smaller quantities, in summer as well as winter, and I am desirous of overrating the expense, rather than otherwise, for fear of any accident in a retort being melted, &c.

Now I do think, the more generally this is made known, that the industrious tradesman may derive from it the benefit he ought, the benefit nature has so bountifully supplied this nation with, the better: especially when candles and oil are risen to so great a price, which is a very great drawback on his profits and industry.

If you can extract any thing from the above imperfect description, that may be of any use, and put it into a shape, so as to make it worth inserting in your Journal, I should be glad. You see what my object is: to show to the middling man as well as the great one, that a considerable saving and advantage may be derived from the use of gas in his manufactory. I have said nothing about the greater safety there is in its use than in that of candles; there being no danger

to

to be dreaded from snuffs and sparks: a circumstance from which I should think the Insurance Offices would be great advocates for its introduction also.

I will also, if you think proper, send you a plain description of a small apparatus sufficiently and easily explained, that shall enable any man to put it up himself; for the thing is so simple, that with a few plain drawings and explanations almost any man of common abilities may do it. It is often the case, that things of great advantage and use to the community at large are kept back and as secret as possible by individuals, who have had the good fortune to derive much advantage from them: but if any thing useful can be introduced for the benefit of mankind, that man is deserving of thanks, who uses the best means nature has bestowed upon him to disseminate its usefulness aboad. Drawing and description of an apparatus promised.

Dr.			*Cr.*		
Yearly expense in coals and man	£18	4	Twenty weeks at eighteen shillings per week for candles	£18	0
Interest on Forty Pounds	2	0	Oil and cotton for lamps	30	0
Profits per year	30	6	Coaks worth	2	10
	£50	10		£50	10

I reckon nothing for the tar, setting it against any little loss or accidents.

I am, Sir,

Your humble servant,

B. COOK.

Caroline Street,
Birmingham, Nov. 22d, 1808.

REPLY.

I Shall with great pleasure receive and attend to the drawing and description offered by Mr. Cook, whose clear descriptions of matters of fact possess a value, which needs not the addition of my suffrage to recommend them.

W. N.

IX.

IX.

Extract from a Letter to J. C. Delametherie, *on Volcanic Substances, by* Lewis Cordier, *Mine Engineer**.

Work on volcanic substances promised.

I Have again visited the mountains of Auvergne, and have finished certain observations and experiments, which will enable me to present the public with a work on different volcanic productions. The following are some of their results.

Titanium in volcanic sands

"All the ferruginous sands of volcanoes capable of being attracted by the magnet are composed of oxide of iron, and oxide of titanium†.

and most lavas.

"The major part of lavas contain a perceptible portion of oxide of titanium.

Granitoid lavas.

"The porous or massive granitoid lavas of the extinct volcanoes in the exterior of France are composed of feldspar, pyroxene, and titanized iron." On comparing them with the green granitello which is found on the summit of Meisner in Germany, and which Werner places in the first rank of those rocks that he calls secondary greenstone, they appear to be perfectly similar. It is no doubt difficult to conceive, how all the authors who have written on the granitello of Meisner could deceive themselves respecting its composition: and such a mistake is so much the more surprising as this rock has given rise to various commentaries. It is certain however, that it is not formed of feldspar and amphibole, as has hitherto been supposed, but of feldspar, pyroxene, and titanized iron, which are very different. This discovery adds fresh weight to the opinion advanced by Mr. Voigt and several German mineralogists respecting the Meisner. It is extremely probable, that the summit of this mountain is in reality a fragment of volcanic strata,

Summit of Meisner.

Probably volcanic.

* Journal de Physique, vol. LXIII, p. 235.

† We must except those sands however, the base of which is specular iron ore; but these are extremely rare.

X.

X.

Letter to J. C. DELAMETHERIE *on some Granatoid Lavas, by* J. P. D'AUBUISSON*.

Some granitoid lavas composed of feldspar and hornblende.

I Have read with much pleasure in your number for September last the letter of Mr. Cordier, in which this mineralogist communicates to you the principal results of his observations and experiments on volcanic products*. To his third assertion, "the porous or massive granitoid lavas of the extinct volcanoes in the interior of France are composed of feldspar, pyroxene (augite), and titanized iron," I can add, "some of the lavas too are composed of amphibole and feldspar."

Lava from Cantal

I have in my possession a specimen of lava from Cantal, which is composed of, 1st, amphibole in long crystals, very black, perfectly laminar, and exhibiting in the most distinct manner the two directions of the laminæ, cutting each other at an angle of about 124°, which, as is well known is the distinguishing characteristic of the amphibole: 2dly, feldspar in crystals of a vitreous aspect, like that of almost all volcanic products: 3dly, a blackish gray matter perforated with numerous small pores. This matter predominates in the mass; yet in some places the amphibole is more abundant. This lava is a true secondary greenstone; that is to say, one of those found in the secondary trapformation, and which are composed chiefly of amphibole and feldspar.

a true secondary greenstone.

Passes into a basalt composed of its elementary molecule.

If the crystals that constitute the lava of which I speak diminished in size so as to be no longer distinguishable by the eye, and the whole ultimately formed a homogeneous mass, which certainly happens in various parts of the stream from which the specimen I possess was broken off, the result would be a compact black rock, a real basaltes, composed only of the elements of amphibole and feldspar, the same *greenstone* but in a compact state: it would be to it nearly the same as compact limestone is to granular.

* Journal de Physique, vol. LXIII, p. 385.

† See the preceding article.

The

Granitoid of Meisner.

The examination of various greenstones of the basaltic mountains of Germany, of the granitoid of mount Meisner among others, had formerly led me to a similar conclusion, and I perceive with satisfaction, that certain greenstones of Cantal indicate a similar formation. This however does not prove, that there are no basaltes composed of the elements of feldspar and augite confusedly united, much in the same manner as we see certain porphyries with base of petrosilex are nothing but compact sienites, or formed of feldspar and amphibole; while others are compact granites, or formed of the elements of feldspar, quartz, and mica.

Augite formerly confounded with amphibole.

The author of the letter finds it difficult to conceive, how all those, who have written on mount Meisner, should have been deceived respecting its composition. I will show how we may readily account for this. Formerly the augite was considered merely as a variety of amphibole (hornblende). Werner was the first, at least in Germany, who distinguished these two substances, which in many respects resemble each other; but he did not distinguish them till after he had written on mount Meisner, and said, that the granitoid on the summit of that mountain was composed of feldspar and amphibole. Authors have since repeated this assertion, and continued to give the name of amphibole to what is in reality augite. I have pointed out a mistake of this kind some years ago; and I have lately mentioned, that part of what some persons, myself among others, had taken for amphibole, in mount Meisner, was partly feldspar coloured green, and partly augite; but I did not go so far as to assert, that this rock contains no amphibole.

XI.

An Examination of a Stone of the Calcareous Species called "Thunder Pick." By Mr. J. Acton, *of Ipswich. Communicated by the Author.*

Common stones too much neglected.

THE great eagerness, with which newly discovered and rare minerals have been sought after by men of science in order to their analysis, has occasioned the more common ones

to

to be in some measure neglected; so that it is not unusual for persons, who understand the composition of the diamond or other valuable gems, to walk in their fields, and pick up many stones, the nature and use of which they are unacquainted with, though perhaps in a friable state composing considerable part of the soil of which they are the proprietors. And I believe in cases where the more common minerals have formerly undergone examination, such is the present improved state of chemistry, and the consequent greater number and purity of tests and reagents, that it will scarcely be deemed superfluous, to subject them to fresh investigation; particularly if it be done with an ardent view to inquiquiry, and with diligent care and attention to the results.

Even those that have been analysed formerly should be examined afresh.

Influenced by these considerations, I have ventured upon an analysis of a stone of the calcareous species, frequently met with in this country, and called by the common people thunder pick, from the supposition of its falling from the clouds in storms of thunder and lightning. It occurs in crystals weighing from 10 to 1000 grains, of a conic shape, with a cavity at the base extending about a fourth part down the centre of the crystal. Its colour varies from gray, brown, brownish red, to almost black, semitransparent. The nearer they approach to the red colour, the greater is their transparency. I cannot find they abound in any particular place, but are generally discovered solitary by the husbandmen when at plough, or turning up the earth by ditching or otherwise. When scratched with a knife it has a strong alliaceous or urinous smell. Its cross fracture is fibrous, with the striæ diverging nearly as from a common centre. Its longitudinal fracture is glittering, with the striæ parallel. It is moderately hard, and of the specific gravity of 2·663.

Thunder pick described.

Crystals.

Colour.

Generally solitary.

Smell.

Fracture.

Spec. gravity.

a. When heated upon charcoal before the blow-pipe, its colour disappears, but it is infusible.

Infusible alone.

b. With phosphate of soda it is difficultly soluble and fuses into an enamelled bead.

Fusible with phosphate of soda,

c. With borate of soda it dissolves more readily, and fuses into a semitranslucid white globule.

with borax,

d. With caustic soda I could only partially fuse it into a white enamel.

and partially with soda.

Exp.

Action of heat on it.

Exp. 1. A. One hundred grains of thunder-pick in coarse fragments exposed in a platina crucible two hours and a half to a moderate heat lost only four grains, but afterwards exposed to a much higher temperature for an hour lost 42·90 grains.

B. One hundred grains in one piece exposed in a porcelain crucible two hours to nearly a white heat lost 45·90 grains.

A and B. The residue of these two operations, amounting to 112·20 grains, were exposed in a porcelain crucible for four hours more to an intense white heat. When the crucible was taken out and examined, only 102 grains could be collected, as the remainder had united to the crucible, but from its apparent quantity no loss of any consequence could have been sustained. The crucible as well as its Wedgwood cover had suffered a commencement of fusion, and they could not be separated without breaking.

Dissolved in nitric acid.

Exp. 2. A. Wishing to ascertain nearly the quantity of nitric acid requisite to dissolve a certain quantity of thunder-pick, I weighed 100 grains of it in fragments, and introduced to it 100 grains of pure nitric acid of the specific gravity 1·431, and added more acid by ten grains at a time, till the whole was dissolved. Having thus found the quantity of acid necessary to dissolve 100 grains of thunder-pick, I placed it on the balance, and equipoised it on the other scale; 100 grains of thunder-pick were then conveyed into the acid, and the weight of the carbonic acid gas was found to be 42·40 grains.

In muriatic.

B. I repeated the above experiment, substituting a quantity of muriatic acid of the specific gravity of 1·149 with 100 grains of thunder-pick, and nicely adjusting the balance as before, found the weight of the carbonic acid gas given out to be 43 grains.

Oxide of iron & manganese.

C. The nitric solution (A) being now filtered became nearly colourless, and left on the filter the colouring matter of the thunder-pick. I believe a little oxide of iron and manganese, which, when dried, weighed 0·40 of a grain.

Carbonate of lime.

D. The filtered solution being treated with carbonate of potash, carbonate of lime fell down, which when collected and ignited in a crucible weighed 96 grains.

E. To

E. To the again filtered liquor was afterwards added caustic ammonia, when no precipitation ensued; but on treating again with carbonate of potash, it occasioned a cloudiness, which fell down, and when collected and ignited weighed 1·50 grains, which dissolved with effervescence in nitric acid.

Exp. 3. A. To be still further assured of the above experiments being managed in nearly an accurate manner, I put 100 grains of thunder-pick into a small gas bottle, and poured on a sufficient quantity of nitric acid to dissolve the whole. I collected the extricated gas with the mercurial pneumatic apparatus in nicely graduated jars, amounting to 91 cubic inches at the temperature 48° and pressure 29·88, which by the usual following calculation gave 92·63 cubic inches at the medium temperature of the air at 60°, and barometric pressure 30 inches. Carbonic acid gas collected

Rate of expansion for every degree of the thermometer, according to Gay Lussac.

60 mean temp.
48 actual temp.
12 difference.

480)91·00(0·189
480
4300
3840
4600
4320
280

12 × 0·189 = 2·268

91
2·26
93·26

30 —— 29·80 —— 93·26
29·80
746080
83934
18652
3|0)2779·1480

92·63 cubic inches at the corrected temperature and pressure.

If

Medium estimate of the carbonic acid.

If the quantities of carbonic acid gas produced in the foregoing experiments be added together, and the average taken as follows:

42·90 45·90	Extricated by heat.
42·40 43·00	Extricated by nitric and muriatic acids.
4)174·20	
43·55	Average.

And if it be admitted, according to the late accurately conducted experiments of Messrs. Allen and Pepys, that 100 cubic inches of carbonic acid gas weigh 47·26 grains, then by the following calculation 43·55 grains will amount to 92·14 cubic inches, which is within half a cubic inch of the gas actually produced—

47·26——100——43·55 92·63 Gas collected.
100

47·26)435500(92·14 Cubic inches by calculation.
42534
·49 Diff.

10160
9452

7080
4726

23540
18894

4646

The carbonic acid gas collected in the last experiment being submitted to the test of limewater, by means of Pepys' Eudiometer, from 98 to 99 parts out of 100 were absorbed.

The following therefore appears to be the result of the above analysis of 100 grains of thunder-pick:

Average

Average of experiments produced—

Carbonic acid gas	43·55	forming 97·50 grs. carbonate of lime.
Lime	53·95	
Oxides of manganese and iron	·40	
Water and loss	2·10	
	100·00	

On adding to the filtered nitric solution of the second experiment succinate of ammonia, as a test for alumine, no cloudiness ensued. No alumine.

Neither was any effect produced on the addition of prussiate of potash for iron.

I had dissolved 200 grains of the thunder-pick in nitric acid, in order to precipitate it, to say how far it would corroberate the former statements, but being interrupted, I unwarily added the alcali without previous filtering, and afterwards a small portion of it; however I proceeded in collecting it, and the precipitate, after ignition, weighed 192·70 grains, which is certainly a nearer approximation than I could have expected under the circumstances.

XII.

Remarks on a Review of Professor Vince's Essay on Gravitation. In a Letter from the Author.

To Mr. NICHOLSON.

SIR,

I Shall esteem it a favour if you will insert the following remarks upon the Edinburgh Review (vol. XXV) of my *Essay on Gravitation.* Remarks on the Edinburgh review of the Essay on Gravitation.

The fluxion of the elasticity from the change of distance $\dot{x}$ is no solution of the problem I proposed to answer, i. e. "to find the effect of the fluid to impel a spherical body of "infinite magnitude, towards the sun." The reviewer has investigated a point, which has nothing to do with the proposed inquiry; and so little was he acquainted with the subject

subject, that he considers the *accelerative* and *moving* forces of a body as the same thing.

I am, Sir,

Your humble servant,

Cambridge, 24*th Nov.* 1808. S. VINCE.

Some of the hypotheses which have been invented to account for gravitation are so fraught with absurdity, that it was not thought necessary even to state them.

XIII.

An Essay on the Sugar of Grapes; by Professor Proust*.

Grapes contain a distinct species of sugar.

THE grape presents us with a sugar of a new species, the existence of which had hitherto been suspected only in conjunction with those sweet and agreeable substances, that are known to form the basis of the flavour of our fruits. Before I proceed to the examination of this product, the mode of extracting it, its qualities, and the uses to which it may be applied, it will be proper to lay down some general principles respecting sugar. We must first distinguish its species, take a view of the substances that usually accompany it, and examine which of the latter it is essentially necessary to separate from it, in order to apply it to our use, and which may be suffered to remain without any sensible diminution of its qualities. This is the order I shall pursue in my inquiry; and a concise view of these particulars I trust will be sufficient, to enable us to judge whether the species of sugar I announce have all the characters of the genus; and whether, while it possesses the qualities of being wholesome and agreeable to the taste, it be sufficiently abundant to supply our wants.

Order of the inquiry.

On sugar and its species.

The immediate products of vegetables

Nature, while she deposits in the various parts of the vegetable structure those compounds, to which we give the name

* Abridged from the Journal de Physique, vol. LXIII, p. 257.

name of immediate products, frequently modifies them by slight shades, and varies each in so many different species. Thus starch, gum, resin, oil, tannin, extractive, &c., while they retain the principal characters of the genus, to which they give their names, differ in certain respects, and thus give rise to the species which analysis has discovered.

getables variously modified.

Sugar too has its species, and of these I purpose first to speak, as the ideas with which they will furnish us are necessary to understand what is to be said respecting the sugar of grapes. If we compare these species with respect to hardness or consistency, we shall find a striking difference in this respect. We see, that the product of the sugar cane is dry, brittle, and easily crystallized; while the driest manna softens with a slight heat, and sticks to the finger that presses it. We find too, that the syrupy product which we call mucoso-saccarine is a third species, differing from the former in uniting the viscosity of a mucilage to the property of retaining a softness that no drying can destroy.

Sugar has its species. These differ in consistency.

The honey, that bees collect from plants, and in which it is impossible not to recognise one of their immediate products, will give us an instance of two species combined. That it frequently varies in consistence is well known; and it has been long presumed, that it must contain a portion of crystallizable sugar, which has even been affirmed, though never proved: but as this conjecture has been confirmed by the experiments I have lately made, I shall proceed to relate them.

Honey a compound of two species.

The honey collected at Madrid on the heights of Flonda is yellow. It has the transparency and tenacity of a turpentine to such a degree, that we may justly say it is to solid sugar the same thing as a balsam is to a resin. Alcohol dissolves it almost entirely: a few particles of wax separate from it, and it afterward deposits a small portion of a viscous substance, which is soluble in water, precipitable by alcohol, and without any particular taste. This is a true gum. The white honeys, of which I shall soon speak, likewise contain a little.

Yellow honey.

The colour of the former is certainly owing to an extractive principle, which cannot differ much from that of vegetables, for the muriate of tin precipitates it in a yellowish

Owes its colour to extractive.

 lake

lake, while with white honey this muriate scarcely gives any appearance.

Does not crystallize.

The alcoholic solution of this honey left to evaporate spontaneously shows no disposition to afford crystals like those I shall soon mention. Perhaps it contains a little solid sugar, which the liquid sugar retains so strongly as to prevent its separation: but this does not prevent me from considering this honey as wholly, or nearly so, one of the two species of sugar, which I purpose to point out in honeys in general.

Thick honey separates into two portions.

When a honey is very consistent and opake, we find that in time it separates into two parts: one granular, crystalline, and opake, that collects at the bottom of the vessel; while the other, transparent and fluid without the addition of foreign moisture, remains at the top. We find too, that white honeys are most liable to this kind of separation, or that they contain habitually more candy than the yellow.

White honey contains most candy.

White honey treated with alcohol

Presuming, that, though both the species contained in white honey were soluble in alcohol, that which is fluid would be less so than the other. I added alcohol to some white honey of the finest quality from the mountains of Moya. The result of this operation, conducted with some precautions which may easily be supposed, produced the separation of a white powder, which subsisted spotaneously. This powder, separated from the solution and slightly washed with alcohol, ultimately afforded me a powdery sugar, which I left to dry in a moderate temperature. Nothing more remained, but to purify it afresh, to make it into a sirup, and dispose it to crystallize. Its solution in water occasioned the separation of some particles of wax; after which, having boiled it down to the consistency of a thick sirup, I set it by covered with a paper merely. In less than two days, which I scarcely expected, it began to cover the sides of the vessel with white points, whence I judged at once, that I must not expect a crop of common sugar. In fact on the fourth day the sirup was converted almost entirely into granular, hollow crusts, which had risen more than an inch above its level. These I set by a few days to drain, in order that its melasses might be separated as much

A little wax separated.

Crystallized.

as

as possible. The following are the qualities of this new kind of sugar.

It has a considerable resemblance to the head of a cauliflower, is perfectly white, and does not attract moisture. Its sweet, agreeable, and cool taste is less saccharine than that of common sugar, has no resemblance to the flavour of honey, but it leaves on the tongue something I can t describe of farinaceous. It is easy to conceive, that if it were employed to sweeten any thing, more would be requisite than of honey or common sugar. Qualities of the crystals.

If it be burned it diffuses the smell and usual fumes of burned sugar. Alcohol dissolves it without any residuum, and by evaporation it separates afresh into granular concretions. Lastly nitric acid converts it readily into oxalic. The melasses that drains from it is nothing but the second kind of honey, of which I shall speak, mixed with a little gum, which alcohol instantly demonstrates. The fluid portion.

The second kind however must not be considered as perfectly free from common honey, because the solubility of the latter in common honey and in alcohol are two causes, that prevent obtaining an accurate separation of them. We may succeed better by leaving a solution of honey in alcohol to evaporate in the open air, for then the first crystallizes, and leaves the second tolerably pure. The honey of the mountains of Moya for instance, which is of a superior quality, affords in this way thirty-nine or forty per cent of crystals, while by washing in alcohol we separate only five or six and twenty. This not free from solid parts.

The fluid honey obtained in this manner is a sugar, that retains a perfect transparency; and however long we boil it, its appearance will only be that of a thick turpentine. It attracts moisture, and is the second part of the sugar, which formed with the first the honey that has just been examined. Qualites of the fluid portion.

I have not examined their proportion in other sorts of honey, for want of time; but till this inquiry is extended to more, we may deduce from these facts some useful inferences respecting the nature of sugar.

In the first place they show us, that the sugar collected from flowers by the bees is of two species. They show us too, that these species, united in honey, and compared with the General properties.

the sugar we derive from plants, resemble it likewise in two points worthy of remark. The first consists in the two degrees of consistency; the one solid, the other soft, which in like manner divide all the vegetable sugars: the second in the flavour, which is commonly more sweet or saccharine in the fluid honeys and sugars, than in those which are crystallizable.

The solid portions not common sugar.

The solid sugar of honey is not similar to that of the cane, either in flavour or crystallization: but in both these qualities it comes so near the sugar of the grape, that I begin to doubt whether there be much difference between them. Unquestionably it would be an important advantage to society, to be able to separate the two species of sugar, that compose most kinds of honey, in order that each might be employed exclusively for those purposes, to which it is best adapted: and though at present I see no hope of succeeding in this but by the mean of spirits of wine, which would be far from economical, I cannot avoid thinking, that the result would be one step toward the emancipation, for which a great part of Europe is anxious, if the sugar of grapes did not offer itself, to hasten a period so desirable.

Their separation difficult, but the grape may render us independent of the West Indies.

Manna.

Its softness not owing to extractive.

It has long been supposed, that the softness of manna, and the readiness with which it grows moist, were owing to an extractive matter; and that this matter, covering the qualities in which it resembles sugar, must be the cause of its laxative virtue. If however we examine its solution with muriate of tin, we find but very little precipitate: and alcohol dissolves manna completely, contrary to the opinion of Lemeri. This solution, left exposed to the air, dries into a porous mass composed of very slender crystalline filaments and granular particles, which by its lightness resembles a fine white agaric.

It is a distinct species of sugar.

Manna thus refined does not approach the sugar of the cane: its moistness and faint taste are still the same. It is not in its nature therefore, to be any thing but what it has always appeared to us, that is to say, a species of sugar, the characteristics of which are softness, an unpleasant taste, and the medicinal properties for which it is used. To ascertain whether manna likewise have its two species, and be in

this

this respect analogous to honey and other sugars, it would be necessary to analyse some fat mannas, of the purity of which we could be certain; but this is not at present in my power.

A distinguishing character of manna is to form with nitric acid the two acids afforded by gum, sugar of milk, mucilage of linseed, &c.; while honey, which approaches manna in degree of consistency, does not.

Yields oxalic and mucous acids, but honey does not.

Manna abounds in America, according to the report of travellers. Herrera says: "In the season there falls a large quantity of a dew, which coagulates like sugar, and the use of which is so wholesome, that they call it *manna*." Is this our manna? or is it a particular kind of sugar? Father Picolo, one of the first spiritual conquerors of California, likewise asserts, that it exudes from the shrubs abundantly in April, May, and June. In Spain manna is so plentiful, that all Europe might be furnished with it, according to the report of two members of the Academy of Physic at Madrid, who were directed to make the inquiry by the Marquis of Ensenada.

Abundant in America, and in Spain.

There is at present no doubt, that sugar exists in a multitude of vegetables, fruits, roots, and stalks, in the sap of the palm, birch, maple, bamboo, maize, &c.: but we do not yet know, whether that of the beet, from which Achard has professed to manufacture it, and of other vegetables, in which Margraff discovered it, be really of the same quality as that of the cane, or a different species; like those that follow.

Sugar exists in many plants.

It does not appear for instance, that the sugar of the maple is very similar to that of the cane. The juice of this tree commonly affords five per cent of solid sugar: it is to be presumed, that it has likewise its melasses, or sugar of the second species. Travellers say, that it is three or four times as long in dissolving as the sugar of the cane; that it sweetens less; that the latter is preferred to it for chocolate; and that a portion of the latter is mixed with it for confectionary. Hence it should seem, that the sugar of the maple is not so agreeable as that of the cane.

Maple sugar.

We are told too, that in Egypt they extract from the pod of the carob tree a kind of honey, which is much prized by

Sugar from the locust tree or St. John's bread.

the

the Arabs. I know already, that this sugar is not crystallizable, but of the second species; and it contains an extractive matter, which gives it a high colour, and spoils it by a particular flavour, which assuredly would not be relished by the rudest Bedoween in Europe. Its wine much resembles that of melasses, and where no other was to be had, might be drunk without repugnance. It is very intoxicating. The brandy produced from it I have made known already.

Vinous and spirituous liquors from it.

Sugar in various fruits.

A sugar equally crystallizable with that of the cane, but very different from it, exists in the gooseberry, the cherry, and the apricot, the juice of all grapes, and no doubt many other fruits. Its crystals are pulverulent, and so difficult to perceive, that I have not yet been able to observe their shape. This produces the concretions, that are found in raisins.

Sugar of figs,

Figs appear to contain a great deal of crystallizable sugar, since I am informed, that thick crusts of it separate from them in the barrels in which they are kept dry.

of gooseberries and cherries,

The candy that forms among preserved gooseberries and cherries equally belongs to these fruits, and not to the sugar of the cane. These concretions dissolved in alcohol always resume the granular form, which is commonly found in these preserves.

of the apple, quince, medlar,

The first species of sugar does not appear to be formed in the apple, quince, or medlar. Their juices afforded me only the second loaded with gum and extractive colouring matter. It is probably the same with plums, peaches, &c., for candy is scarcely ever found in their jellies or preserves. In all these fruits however the saccharine product is confounded with gum, extractive matter, the malic, citric, and tartarous acids, sulphate of lime, &c.

plum, peach, &c.

The two species variously distributed among fruits.

These facts, which deserve a farther examination in the vegetable kingdom, contribute still more to confirm the existence of the solid and mucoso-saccharine species of sugar which appear to divide our fruits in very various proportions.

The fluid species.

The fluid sugar, which had received the compound name of mucoso-saccharine, because it was considered as a mixture of solid sugar and mucilages, was not well understood by

by any one before Deyeux: He perceived, that it was a species of the genus, habitually fluid, and to be classed among the immediate products. He judged too with reason, that of the two species of sugar known it was the only one capable of fermenting by itself, while the other will not undergo this change unless disposed to it by some ferment. The only one that will ferment spontaneously.

The labours of Duthrone too has so clearly confirmed the existence of this product by the various facts he has collected in his work, as to render it no longer disputable, that the only object of the sugar-makers must be, to separate the crystallizable sugar from the fluid: but I shall give here the results of the analysis I have begun of the sugar-canes of Malaga. To separate this from the solid the only object of the sugar maker.

In the juice of these, when fresh drawn, we find green fecula, gum, extract, malic acid, sulphate of lime, and the two species of sugar. All these products, except as to their varieties, are the same as those met with in most fruits. Juice of Spanish sugar canes examined.

A slice of the cane put into infusion of litmus reddens it powerfully: yet its juice is not perceptibly acid to the taste, because the acid in it is only in a very small quantity, and the sugar covers it; but in the juice boiled down it is plainly perceivable. The following are the effects of reagents on it. Contains an acid,

The oxalic acid and barytes form a copious precipitate with it, which proves, that it contains sulphate of lime. Concentrated solution of platina throws down nothing so that it contains no salt with base of potash. sulphate of lime, no potash,

Alcohol poured on the sirup of the cane separates from it insoluble filaments, which fall to the bottom of the vessel, and are pure gum. Some time after the gum a small portion of a white powder falls down, which is sulphate of lime. This single product classes the juice of the cane with that of most fruits. gum,

The sirup, when freed from the gum and sulphate, forms copious precipitates with the nitrates of lead and silver. With limewater too a precipitate is formed, and the liquor turned green. This indicates the presence of extract, which muriate of tin confirms by precipitating a whitish lake. The same extractive matter.

same sirup distilled with weak sulphuric acid affords no signs of vinegar; the acid it contains therefore is not volatile.

and malic acid.

If this sirup be boiled with chalk, its acid is saturated; and from the filtered and concentrated juice alcohol separates malate of lime, but in so small a quantity, that we need not be surprised if Macquer and Darget, in the experiments they made at Bercy near Paris, did not perceive any acid in the juice of the cane. Duthrone, when he said, that the repeated employment of potash and lime in the clarification of sugar must have for its object, to saturate any thing but acids, was in the right. He even thinks, that the alkalis combine with some remains of glutinous fecula, and thus lessen their too great solubility. Yet as it seems difficult to conceive, according to our ideas of the properties of gluten, how it can remain dissolved in so large quantity in juices void of acid, or deprived of it by the first saturation, I dare not at present adopt this opinion, because I do not see clearly what is the use of the alkalis in the clarification of the juice of the sugar-cane.

The use of repeatedly adding potash and lime in purifying sugar, not ascertained.

Infusion of the fresh cane

The cane cut into thin slices gives out its soluble parts to water. On concentrating this solution, a little before it boils a greenish, feculent film separates, which does not differ from that of gooseberries, grapes, &c., and affords abundance of ammonia on distillation. Duthrone obtained the same result.

yields ammonia.

Boiled down to muscovado.

If it be boiled down to the consistence of a thick sirup, in fifteen or twenty days a congelation like honey will be produced, sufficiently firm to remain fast in the vessel.

Its flavour.

The taste of this muscovado sugar is pleasant: and it has an aromatic flavour, which may be better recognized in melasses, or still more in rum. The race of this liquor therefore is really that of the cane: it is a product of the plant, and not a precipitate formed in the preparation, or by any of the changes to which the juice is exposed before it arrives at the state of melasses.

The cane affords half its weight of juice.

According to Duthrone, the cane commonly affords half its weight of juice. This juice marks on Baumé's areometer from 5° to 14°, a difference depending on the ripeness, and the influence of other causes, which occasion an increase or

or diminution of the products of the cane, as of other plants. According to him 14° indicate twenty-five pounds eleven ounces of sugar to a hundred pounds of juice; and, as in the most favourable circumstances the cane does not yield above half its weight of juice, a hundred pounds of the cane cannot produce more than thirteen of raw sugar. If we speak of refined sugar, this product must be reduced at least one third; since raw sugar appears to contain not much less than this proportion of melasses. The proportion of dry to liquid sugar however is yet to be ascertained. No doubt it will vary according to the strength of the plants, but it deserves to be inquired into, and I shall attend to it when I resume my examination of the canes of Malaga. To return to the muscovado, or raw sugar.

and at most 13 per cent of sugar, or about eight of refined.

When we consider this honeylike mass, such as it is afforded by the evaporation of the juice, that is with its sweet and agreeable taste modified by the slight bitterness of its extractive principle, we may reasonably conjecture, that the oriental nations, after they had discovered it, and placed it among the condiments adapted for seasoning their insipid rice, would employ it for many centuries in this state, as they did honey; and we may presume it was from the resemblance of honey to the raw sugar then in use, and not to refined sugar, that some of the ancient naturalists termed sugar "another kind of honey, that is formed in reeds." Honey itself, the only production that has any real resemblance to muscovado, not being capable of any process of refinement to improve its qualities, they would naturally continue long of the opinion, that raw sugar was equally insusceptible of that degree of perfection, to which it has been brought in modern times: and if we consider the number of ages that elapsed, from the time when corn began to be the general food of man, to that in which he discovered the art of making fermented bread, we shall find this conjecture respecting raw sugar extremely probable. Besides, it is proved by the historical researches of Duthrone, that toward the end of the fourteenth century raw sugar, without any farther purification, was an article of trade in Egypt, Syria, Cyprus, &c.

Muscovado long in use in the East without refining.

But if the refinement of the honey of the cane have happily

Part of the

sugar is lost by refining.

pily enabled us to enjoy the use of sugar in all its purity, we must confess, that we do not obtain this advantage without sacrificing a part of the saccarine matter it contains: for it is certain, that, if the melasses, which probably amount to more than a third, could likewise be deprived of the extractive matter concentrated in it by evaporation, as well as by the several preparations it undergoes, with the foreign matters introduced into it by the potash, lime, and bullock's blood, we should have in it a sirup, which, notwithstanding the inconvenience of its fluidity, would be a very useful substitute for sugar, in all cases where the luxury of our tables does not render the latter indispensible. And it would have the farther advantage of sweetening in smaller quantities: at least I may reasonably infer this from the melasses I have separated from raw sugar, the qualities of which render it far superior to the melasses of our sugar refiners, as it is not contaminated by any foreign mixture.

(To be concluded in our next.)

SCIENTIFIC NEWS.

An Essay on the Warming of Mills, and other Buildings, by Steam. By ROBERT BUCHANAN, *civil Engineer. Glasgow.*

Buchanan on warming buildings by steam.

IN this little but valuable pamphlet Mr. Buchanan has collected the principal facts relative to the application of steam for the purpose of communicating heat. There are two points of view in which this subject may be considered, safety and economy. In large manufactories of combustible articles the safety arising from the exclusion of coal fires must be an obvious advantage. How far it may be economical must depend greatly on local circumstances.

History.

The idea was suggested by Colonel Wm. Cook, in the Philosophical Transactions for 1745, but it does not appear to have been applied practically. Mr. Snodgrass first applied it to the warming of cotton works in 1798; see our Journal, vol. XVI, p. 326: and his example was followed by others.

Heads of the subject.

Mr. B. arranges his subject under the following heads: 1. The proportionate size of boilers and quantity of fuel. 2. The

2. The proportion of steampipe required to heat a given space. 3. The substance and colour of steampipes. 4. Their direction and arrangement. 5. The modes of connecting them.

1. A cubic foot of boiler will heat about 2000 cubic feet of space in a cotton mill, where the temperature is in general from 70° to 80° of Fahrenheit. The boiler is supposed to be of the shape commonly used for a steam engine, and 25 cubic feet to be equal to a horse's power. Where the boiler is separate, and not used for the joint purpose of working an engine and warming a building, it should be considerably larger than in this proportion, to avoid the inequality of heat incident to a boiler working at its full capacity: 25 cubic feet of boiler require about 14lbs. of good Newcastle coal per hour. Size of boilers and quantity of fuel.

2. Every square foot of exterior surface of steampipe will warm about 200 cubic feet of space in a cotton mill. A small chapel has been warmed comfortably with half that proportion. The safety valve in the boiler is supposed to be loaded about $2\frac{1}{2}$ lbs. to the square inch. If the steam were stronger, it would give more heat, but it would be difficult to keep the joints of the pipe steam tight. Proportion of pipe to space.

3. Cast iron pipes give out above twice as much heat as copper, or tin, unless the tin be painted black. When the surfaces are equally dark, and equally rough, there is no apparent difference. The thicker the pipe the more uniform the temperature; but on account of the expense they are generally not more than $\frac{1}{2}$ or $\frac{1}{4}$ of an inch thick. Materials of pipes.

4. The expansion of cast iron pipes may be estimated at $\frac{1}{10}$ of an inch for every 10 feet in length. Vertical pipes, being equally heated all round, continue straight; but horizontal pipes bend, because the upper side is heated most, and this endangers the joints. Vertical pipes too may be used as pillars for supporting the floors. In the arrangement of the pipes two points require considerable attention. First, conveniently to expel the air; and, secondly, to take off the water proceeding from the condensation of the steam. When the steam enters the pipes, it acts as a piston, driving the air before it. This principle should be kept in view in fixing on the place of the opening for the escape of the air. Their arrangement.

After

After the pipes are filled one or more openings will be necessary, to allow a small portion of steam constantly to escape, to keep up the heat of the pipes. If this be not done, air will accumulate, and occupy the place of the steam. It is best to make the condensed water run in the direction of the steam, which will drive the water before it in a horizontal pipe, or even in one with a considerable acclivity. Great care must be taken however, that no water lodges in the pipes: for, the water remaining in the pipes after they become cool keeps one part of them cold; the next time the steam is let into the pipes the regular expansion is prevented, some part of the pipe cracks, and a violent explosion takes place, racking the joints to a considerable distance in every direction. The common arrangement is to have a horizontal pipe going off separately from each vertical pipe. This requires an opening for letting out the air at the end of each horizontal pipe. A great improvement is first to carry the steam to the upper story in a vertical pipe, and close under the ceiling in a horizontal one nearly to the opposite end of the building; thence in a vertical pipe to the story beneath, and again horizontally under the cieling; and thus from story to story till it comes to the bottom; where the condensed water may be allowed to run off by an inverted siphon, which will allow the water to escape, while its pressure confines the steam. The air may be allowed to escape by a stopcock at the same place.

Mode of connecting them.

5. When the joints are formed by bolted flanches, these are liable to be broken from inequality of expansion, or to leak at the bolt-holes. Spigot and faucet joints do very well in some cases, but sometimes the faucets burst, from the greater expansion of the spigots. If the ends of both pipes be included in thimbles, though these are equally liable to break, the expense is trifling compared with that of a whole pipe. For branch pipes the joinings should be made by saddles and hoops embracing the main pipe. Where there is much silk of unequal expansion, the joints should be secured by a soft stuffing of hemp, or cotton, and tallow; but in most cases they may be made with iron cement, composed of ion borings 40lbs, sal ammoniac 1lb, sulphur ½lb, well mixed together and beaten like putty.

The

The following is a Tabular View of the Effects of Steam in warming different Buildings.

Buildings.	Substancce of which the steam-pipes are made.	Cubic feet of space in building.	Cubic feet in boiler.	Space warmed by 1 cubic foot of boiler.	Cubic feet of space warm ed by 1 superficial foot of steam-pipe.	Tempperatures, degrees of Fahrenheit in winter.
Messrs. H. Houldsworth and Co. Anderston Old Mill, ...	Cast.Iron,	250000	——	2000	178	85°
Linwood, Ditto, ...	Cast Iron,	300000	120	2500	168	70°
Messrs. Kennedy and Watts, Johnston, ..	Cast Iron,	289000	160	1180	160	75°
Catrine, ...	Tin-plate not painted,	——	——	——	200	——
Mr. Thomas Houldsworth's Mill, Manchester, ...	Cast Iron,	——	——	——	195	——
Chapel of Port-Glasgow, ...	Cast Iron,	60600	10	6000	400	——
Part of Adelphi Cotton Works, ...	Cast Iron,	49140	——	——	182	65°
Tambouring Mill at Anderston, ...	Cast Iron,	——	——	——	240	60°
Messrs. William King and Sons, Johnston	Cast Iron,	244583	180	1303	200	70°

METEOROLOGICAL JOURNAL

For NOVEMBER 1808,

Kept by ROBERT BANCKS, Mathematical Instrument Maker, in the STRAND, LONDON.

Day of OCT.	THERMOMETER. 9 A. M.	9 P. M.	Highest.	Lowest.	BAROMETER, 9 A. M.	WEATHER. Night.	Day.
26	48	44	52	42	29·25	Rain	Rain
27	44	44	50	44	29·42	Fair	Ditto
28	44	45	50	43	29·39	Ditto	Ditto
29	44	44	49	42	29·66	Cloudy	Ditto
30	46	47	48	42	30 03	Ditto	Ditto
31	44	44	50	44	30·34	Fair	Cloudy
NOV.							
1	48	47	50	46	30·26	Ditto	Ditto
2	48	48	51	46	30·15	Cloudy	Ditto
3	47	47	50	42	30·04	Ditto	Ditto
4	44	42	47	38	30·21	Rain	Fair
5	40	37	41	32	29·90	Fair	Ditto
6	34	36	42	38	29·88	Ditto	Ditto
7	42	43	43	42	29·70	Cloudy	Ditto
8	46	48	50	45	29·65	Rain	Rain
9	47	50	52	50	29·56	Cloudy	Ditto
10	48	48	52	48	29·74	Ditto	Fair
11	46	45	48	38	29·84	Fair	Rain
12	42	41	44	40	30·10	Cloudy	Fair
13	40	38	41	32	30·11	Ditto	Ditto
14	34	37	38	34	30·11	Fair	Ditto
15	40	47	49	42	29·98	Cloudy*	Ditto
16	50	51	54	49	29·61	Ditto†	Ditto
17	50	51	51	46	29·27	Rain‡	Rain
18	46	43	48	33	28·80	Ditto §	Ditto
19	34	38	40	36	29 31	Cloudy	Ditto
20	40	46	50	45	29·73	Rain	Ditto
21	50	46	54	40	29·76	Fair	Fair
22	41	41	52	46	30·18	Ditto	Rain
23	47	50	52	46	30·12	Rain‖	Cloudy
24	46	45	50	44	30·19	Ditto	Ditto
25	48	51	52	46	30·00	Fair	Ditto

* 11 P. M Rain and high wind.
† Ditto ditto.
‡ Ditto ditto.
§ Rain and fall of snow.
‖ Preceded by heavy mist.

A JOURNAL OF NATURAL PHILOSOPHY, CHEMISTRY, AND THE ARTS.

SUPPLEMENT TO VOL. XXI.

ARTICLE I.

Essay on the Composition of Alcohol and of Sulphuric Ether. *By* THEODORE DE SAUSSURE.

(Concluded from p. 273.)

Decomposition of Sulphuric Ether.

SECT. VI. *Preparation of the Sulphuric Ether employed in my Experiments. Considerations on the Analysis of this Fluid.*

A HUNDRED parts of sulphuric acid by weight, mixed with a hundred parts of spirit of wine of the shops, the specific gravity of which was 0·842 at 16° of Reaumur [68° F.], afforded me by distillation through a worm 53 parts of ether not rectified, the specific gravity of which was 0·797. Preparation of the ether.

This ethereous liquor, after mixing it with an alcoholic solution of potash, was distilled nearly to half at a temperature of 35° R. [111° F.]. The ether, freed from the sulphurous acid, oil, and a part of the alcohol which were united with it, was of the specific gravity of 740° at 16° R. [68° F.]. This is the rectified ether of the shops. Rectified.

Washed.

The ether obtained by this operation was mixed with twice its weight of water *, to separate still more alcohol from it: and the ether, after it was decanted, was found to be reduced by this washing to the specific gravity of 0·726.

One third drawn off.

This last product subjected to distillation, and only a third of it drawn off, yielded an ether of the specific gravity of 0·717 at 16° R. [68° F.]. This was the ether I employed in my experiments.

More might have been obtained.

It is unnecessary to observe, that by repeatedly rectifying, and washing the residuums of the preceding rectifications, four or five times as much ether of the specific gravity of 0·717 might be obtained, as by confining ourselves to the first result mentioned above.

Results of burning in a lamp not accurate.

It has been seen, that the results of the slow combustion of alcohol in a lamp in a close vessel were deficient in precision. Those I obtained by the slow combustion of ether were still more so, and therefore I shall not detail them.

Its detonation gives more specific results than that of alcohol.

The analysis of ether made by detonating its elastic vapour appeared to me sufficiently accurate to determine the proportions of carbon, oxigen, and hidrogen. It is capable of giving more precise results than those obtained from alcohol by this process. The alcoholic vapour is so light, that its specific gravity is difficult to be ascertained. A very slight errour in determining it makes great differences in the results of the analysis. The gaseous vapour of ether is much heavier; all the results are more striking; and small errours here are less important.

Its decomposition in a red hot tube not quite so accurate.

The decomposition of ether by an incandescent porcelain tube afforded me less precise results than the preceding operation, and much less accurate than those obtained from alcohol by the same means, because the ether in this process yields thirty times as much oil, with respect to the composition of which I could only form conjectures. I will relate the particulars of this process however; as they will serve to confirm the analysis of ether by the rapid combustion of its vapour.

* The efficacy of this method has been demonstrated by the experiments of Gay Lussac, given in Berthollet's Chemical Statics, vol. I.

SECT.

Sect. VII. *Decomposition of Ether by an incandescent Porcelain Tube.*

1103 grains of ether passed through a red hot porcelain tube,

Through a porcelain tube glazed within and heated red hot I passed 1103 grains of ether. I did not apply fire directly to the retort, from which the ether was distilled, for the vicinity of the furnace that heated the tube raised it to 27° R. [92·7° F.], and this temperature was sufficient to distil over the whole of the ether in the space of fourteen hours.

as the alcohol had been,

The apparatus for this experiment was in all respects similar to that employed for the analysis of alcohol, and described in Sect. V. The porcelain tube was equal in size, and exposed to the same degree of heat in the same furnace.

was entirely decomposed, yielding

The ether was entirely decomposed: at least no smell of ether was perceptible in the vessels, that received the products of the operation. It yielded me,

charcoal 5¼ grains,

1, In the middle of the porcelain tube 5¼ grains of charcoal, which separated in the form of a thin leaf or scroll. This charcoal, being incinerated in a platina crucible, left no ponderable quantity of ashes.

volatile oil 3 grains,

2, In the glass worm and the upper half of the receiver about three grains of a very inflammable essential oil, crystallized in shining scales, transparent, and smelling of benzoin. Most of these crystals were contaminated by a brown empyreumatic oil, which they left behind after evaporating in the common temperature of the air.

another oil 43 grains,

3, In the end of the porcelain tube that projected beyond the furnace, in the worm, and in the receiver, where it was more abundant, 43 grains of an oil nearly black, partly fluid, and partly of the consistence of honey. This had a smell of benzoin mixed with an empyreuma; was soluble in alcohol, and insoluble in water; acrid, and, the lips being touched with it, it gave pain, and caused suppuration. When spread upon paper it dried, and, viewed through a microscope, exhibited small yellow crystals, which were not volatile like the preceding in the common temperature of the atmosphere.

water 3 grains,

4, A drop of water, weighing about three grains, found in the worm. It was colourless, smelt of benzoin, emitted

results we have just obtained from the detonation of the oxicarburetted hidrogen gas of ether, we find, that 100 parts by weight of the latter contain,

Carbon - - -	56·12
Hidrogen - - -	17·43
Oxigen - - -	26·45
	100.

Ether contains more carbon and hidrogen, but less oxigen, than alcohol.

The analysis of this gas compared with that of alcohol is sufficient to give us some idea of the composition of ether, and show us, that this liquor contains in an equal weight more carbon and hidrogen, but less oxigen, than alcohol: for this oxicarburetted hidrogen gas alone constitutes more than three fourths of the weight of the ether I decomposed. The other fourth, which I pass by, is almost wholly oil, in part fixed, in part volatile, which must have some similarity of composition with the ether. But as oil, according to the analysis of Lavoisier, contains scarcely any thing but carbon and hidrogen, it follows, that, in referring the composition of ether to the proportions of the elements of the gas I have just analysed, we have too little hidrogen and carbon, and too much oxigen. This will be confirmed by the following process, which gives more precise results.

SECT. VIII. *Analysis of Ether by the Detonation of its Elastic Vapour.*

Vapour of ether detonated with oxigen gas.

For the preparation of the oxigen gas dilated by the vapour of ether, and the estimation of the weight of this vapour, I adopted the same processes as those I applied to the vapour of alcohol, § III. I think it useless therefore to repeat them; but I shall give one example of their results, the barometer being at 27 inches, and the thermometer at 18° [72·5° F.]. Of five experiments made in a similar way this appeared to me the most accurate, though their differences were slight.

Expansion of the vapour.

The elastic force of my ether, or the depression of the column of mercury by a drop of this fluid introduced into its vacuum, was 16 inches 9 lines. On applying to this result the formula of Dalton $\frac{p}{p-f}$, we find, that a volume of

air

air equal to unity, into which the ether is introduced, will occupy, in consequence of the expansion of the ethereal vapour, a space equal to 2·6341. I obtained the same result by passing a drop of ether into a receiver full of air over mercury, and measuring this air both before and after the dilatation.

A thousand inches of atmospheric air dilated by the vapour of ether contained therefore 379·63 cubic inches of pure atmospheric air, which weighed 161·9 grains.

1000 cubic inches weigh 654·47 grains.

I found by a direct experiment, that 1000 cubic inches of atmospheric air dilated by the vapour of ether weighed 816·37 grains.* Consequently 1000 cubic inches of pure ethereal vapour weigh in atmospheric air 816·37 − 161·9 = 654·47 grains, according to the principle, that elastic vapour has the same weight in the air and in vacuo. (*See note at the end of this paper.*)

Oxigen gas dilated as much as possible with vapour of ether or of alcohol is not fired by electricity.

Oxigen gas dilated as much as it can be in the common temperature by the vapour of ether will not take fire by the electric spark. The reason is, the vapour of ether is too abundant, or, in other words, the oxigen gas too much rarefied. Alcoholized oxigen gas likewise does not take fire, but from an opposite cause, the alcoholic vapour being too much rarefied; for, on adding pure oxigen gas to the alcoholized oxigen, the vapour does not take fire, because it is still more rarified; but if pure oxigen gas be added to the etherised oxigen, the vapour inflames.

Vapour of ether fired with a large proportion of oxigen.

I mixed over mercury 100 parts by measure of etherised oxigen gas with 504 parts of oxigen gas, and detonated them by the electric spark. The explosion burst the eudiometers, which were not very thick. The 604 parts of aeriform fluid, which before the detonation contained 541·96 parts of oxigen gas, were reduced by their combustion to 344·31 parts, in which a second eudiometrical analysis showed there were 230·51 parts of carbonic acid gas, and 113·8 of oxigen gas. The residue of the first operation contained a dew, which appeared to be aqueous, and was void of smell.

Produce of 65·447 grains of vapour of ether.

One hundred parts by measure of vapour of ether therefore consume 428·15 parts of oxigen gas*, leaving as a residuum

Soot appears in some cases.

* If with etherised oxigen gas we mix a less quantity of oxigen gas

residuum water, and 230 51 parts of carbonic acid gas. Hence we must conclude, that the oxigen gas has burned 395·28 parts of hidrogen gas contained in the ether.

Admitting the numbers I have given to represent cubic inches, and substituting for these the corresponding weights, the barometer being at 27 inches, and the thermometer at 18° [72·5 F.], we find, that 100 cubic inches of ethereal vapour weigh 65·447 grains, and contain,

Carbon. 1, The carbon of 230·51 cubic inches of carbonic acid gas, or 38·64 grains of carbon.

Hidrogen. 2, Hidrogen gas 395·28 cubic inches, weighing 12·62 grains.

Water. 3, A quantity of oxigen and hidrogen answering to 14·187 grains of water.

Its component parts. On substituting for the water its elements, and proportioning all the results of the analysis to 100 parts of ether by weight, we find, that they contain

Carbon - - - -	59·04
Hidrogen - - - -	21·86
Oxigen - - - -	19·1
	100.

General results. These results are reducible to the following: 10 grains of ether consume for their combustion 61 cubic inches of oxigen gas, at 28 inches of the barometer, and 10° of the thermometer [54·5 F.], forming water, and 32·85 cubic inches of carbonic acid gas.

The analysis I have just related was repeated four times, and a mean of the four indicates in 100 parts of ether

Carbon - - - -	58·2
Hidrogen - - - -	22·14
Oxigen - - - -	19·66
	100.

gas than will consume all the ethereal vapour, or merely sufficient for this purpose, a black powder, or soot, will be deposited on the sides of the eudiometer, and some free oxigen gas will remain in the aeriform residuum of the detonation. This soot does not appear, when the etherised oxigen gas is detonated with a quantity of oxigen gas much superior to what is requisite for burning the whole of the ethereal vapour.

SECT.

Sect. IX. *Examination of the Water produced by the Combustion of Ether.*

Hitherto I have taken it for granted, that the fluid residuum of the combustion of ether was water, but without any other proof, than a very superficial examination of the slight dew, which is formed in the eudiometer by the inflammation of the ethereous vapour. It remains for me to examine how far this supposition was well founded. Examination of the water

I burned several ounces of ether, in the apparatus invented by Meusnier to obtain the water produced in the combustion of alcohol. The water thus obtained from ether is without colour, smell, and taste, except some traces of empyreuma, which it loses by exposure to the air. It has the same specific gravity as distilled water, with which it mixes without becoming turbid. It is not precipitated either by nitrate of silver, lime water, or even acetate of barytes. When I evaporated one ounce of it to one fifth of its former weight, acetate of barytes produced a cloud in it incapable of being weighed. from several ounces of ether.

To estimate the quantity of sulphur contained in sulphuric ether by another process, I dissolved one ounce of this liquid in fourteen ounces of water. A stream of oxigenized muriatic acid gas was passed through this solution for ten hours. The ether was in part decomposed, but the solution containing the products of this decomposition was rendered but slightly turbid by acetate of barytes, till it was reduced by evaporation to a quarter of an ounce. As the result is so trifling, it is impossible to conceive, that sulphuric ether can derive any of its essential properties from the presence of sulphur. Examined for sulphur.

The water obtained from ether by the apparatus of Meusnier was rendered turbid and of a deep brown colour by the hidrosulphuret of potash. This precipitate arose from the lead acquired from the worm of the apparatus. A little lead in the water from the worm.

It emitted copious ammoniacal fumes at the approach of muriatic acid, and it appeared to me, that it changed the sirup of violets green in a very slight degree: but this change of colour certainly did not take place with the water obtained from the combustion of ether under the mouth of a Appearance of ammonia.

 glass

glass jar. In the latter process the distillation is slower, because we lose a larger quantity of water raised in vapour; and that which is collected, having been longer exposed to the air, suffers more ammonia to fly off.

Further proof of its presence.

One ounce of the water obtained from ether in Meusnier's apparatus, and received in a bottle into which I had put a few drops of muriatic acid, in order to saturate the ammoniacal vapours during distillation, was evaporated to dryness in the temperature of the atmosphere. The residuum it left was dry and well crystallized muriate of ammonia, but mixed with a little muriate of lead. The muriate of ammonia, separated from the metallic salt by a fresh solution and crystallization, weighed one grain and three tenths. Here therefore its proportion was greater than in the water obtained from the combustion of alcohol, § IV.

Perhaps the ammonia not wholly from the ether.

Though it is possible, that ether may contain a little nitrogen, I doubt whether the ammonia, found in the aqueous product of the combustion, come wholly from the ether. Whatever care I have taken in my eudiometrical trials, I have not been able to satisfy myself, that the nitrogen gas is not condensed into ammonia in the combustion of the vapour of ether. My results on this point have not been uniform. The greater number have indicated this condensation, and I am inclined to admit it, because the manipulations and slight errours incident to Volta's eudiometrical process have a tendency to produce the opposite effect, in other words, to introduce nitrogen gas into the residuum of the detonation *.

The water evaporated left a little residuum.

I evaporated to dryness, in a very gentle heat, 288 grains of water obtained from ether burned under the mouth of a glass jar. It left as a residuum a transparent varnish, weighing at most an eighth of a grain, and attracting moisture from the air.

* If we operate over mercury, there is always in this metal, and some interstices of the eudiometer, a little common air, which mixes with the residuum of the detonation, to fill the vacuum it produces. When the operation can be performed over water, the air contained in this fluid separates from it for the same reason, but it is in less quantity than from mercury.

To

To find whether the liquid I examined contained acetic acid, I added a few drops of potash to 288 grains of water obtained by the same process as the preceding. The solution was saturated with carbonic acid gas, then evaporated to dryness, and afterward washed with alcohol; when a white salt was dissolved, weighing 0·7 of a grain, and very speedily deliquescing. It had all the characters of acetate of potash.

It contained a little acetic acid.

Thus the experiments I have just related indicate in the water produced from the combustion of ether the presence of acetate of ammonia, a portion of sulphuric acid too small to be weighed, and a slight deliquescent varnish, the nature of which I could not ascertain. But the weight of all these substances taken together is so small with respect to the water holding them in solution, that it can make very little difference in the proportions of carbon, hidrogen, and oxigen, assigned to ether in my last analysis.

All the foreign matters in it trifling.

Sect. X. *Application of the preceding Analyses to the Inquiry concerning the Changes Alcohol undergoes in its Transformation into Ether.*

In considering the changes effected in the conversion of alcohol into ether, I shall regard only the proportions of oxigen, hidrogen, and carbon, neglecting the nitrogen; the existence of which in alcohol is certain, but questionable in ether, though it is demonstrated, that the water produced by the combustion of ether with the acid of atmospheric air contains a perceptible quantity of ammonia.

Comparison of alcohol with ether.

	100 parts of alcohol are composed, § V, of	100 parts of ether, § VIII, of
Carbon	43·5	59
Oxigen	38	19
Hidrogen	15	22
Nitrogen	3·5	
	100	100

These results show, that in equal weights ether contains much more carbon and hidrogen, but much less oxigen, than alcohol does. Mr. Berthollet had already considered

Alcohol loses oxigen in becoming ether.

ether

ether as a product, that must have more hidrogen and less oxigen than alcohol*.

Residuum from the mixture of sulphuric acid and alcohol.

The residuum of the mixture of sulphuric acid with alcohol holds in suspension, after the ethereous fluid is separated, a bituminous or resinous matter † greatly loaded with carbon. It will be asked, no doubt, how it is possible, that ether should contain more carbon than alcohol, since the latter has let fall a portion of this element in its conversion into ether. But it must be remembered, that this residuum contains likewise oxigen and hidrogen, which are found either in the bituminous substance or in the state of water; and that, if this oxigen and hidrogen be taken from the alcohol in larger proportion than the carbon, the latter must predominate in the ether.

Two parts of alcohol yield one of ether.

To judge whether my analyses lead to this conclusion, I have examined what quantity of ether a determinate weight of alcohol would produce; and I found by approximation, that two parts of alcohol, if wholly decomposed, would give one of rectified ether. I obtained this result by the following operations.

80 p. alcohol, 20 water, and 100 sulphuric acid, produced 60 of impure ether.

A hundred parts of common spirit of wine of the specific gravity of 0·845, and containing 80 parts of perfect alcohol with 20 of water, produced, by mixture with an equal weight of sulphuric acid, 60 parts of ethereous fluid not rectified, by stopping the distillation at the moment when the sulphurous smell is decidedly perceived, and the oil begins to appear. I actually collected only 53 parts of the ethereous fluid, but I found, from the difference in the weight of the retort, that contained the spirit of wine and sulphuric acid, taken before and after the distillation, that 60 parts had been produced. It is well known, that a certain quantity of ether always flies off in vapour during this process, the weight of which could not be found in any other way. In the following operations I continued to estimate the weight of the product by that of the residuum.

* Statique Chimique, vol. ii, p. 531 and following.

† *Ibid.* and Proust, Mémoires des Savans étrangers, Institut nat. vol. I,

The

These rectified to 25·25.

The 53 parts of ethereous fluid, which I suppose equal to 60, were mixed with liquid potash, and by the known processes of rectification afforded me 25·25 parts of ether.

Residuum yielded 10·3 more,

The residuum of this rectification, which must be equal to 34·75 parts, was separated from the potash by distillation. It was miscible with water in any proportion, and had nearly the specific gravity of common spirit of wine. I mixed it with an equal weight of sulphuric acid, and it produced 23·25 parts of ethereous fluid, which, having been mixed with potash and rectified, yielded 10·3 parts of ether.

and afterward 3·2.

The alcoholic residuum of this rectification was separated from the potash, and mixed for the third time with sulphuric acid. This afforded 3·2 parts of rectified ether. The 80 parts of perfect alcohol therefore produced in all these operations $25{\cdot}25 + 10{\cdot}3 + 3{\cdot}2 = 38{\cdot}75$ parts of ether, or nearly half the weight of the alcohol employed. Ten parts of water did not entirely dissolve one of this ether. Its specific gravity was equal to 0·736 at 16° R. [68° F.]. I did not wash it with water, but it would certainly have been lighter, if I could have obtained the specific gravity of that which was volatilized. I have taken no account of a small quantity of spirit of wine, which, according to the observation of Proust, always remains mingled with the sulphuric acid after the first separation of the ether. I do not think therefore I shall be far from the truth, if I say, that 200 parts of perfect alcohol produce by their complete decomposition 100 parts of ether of a density equal to 0·717 at 16° R. [68° F.].

In all 38·75 of pure ether

Elements left by the alcohol after the separation of the ether.

If we take the difference between 200 parts of alcohol and 100 of the ether produced from it, reducing the two liquids to their ultimate elements, we shall have a remainder equal to 100 parts, which, setting aside the sulphuric acid, express the elements left by the alcohol after the separation of the ether; and which include

Carbon	-	-	-	28
Oxigen	-	-	-	57
Hidrogen	-	-	-	8
Nitrogen				

This

This residuum then must contain a considerable quantity of carbon, though the ether is more loaded with it than the alcohol. It will no doubt be remarked, that this residuum contains oxigen and hidrogen nearly in the proportions that constitute water, or in that of 7 to 1. We must admit therefore, that 100 parts of ether are nearly equal to 200 parts of alcohol, minus 28 parts of carbon, and 65 of water, the formation of which has been occasioned by the sulphuric acid.

Not a perfect separation of the products.

The black substance precipitated from the alcohol is not, as has been said, pure carbon: nor does it appear, that the liquid formed with the elements of alcohol by the sulphuric acid is pure water. An imperfect separation of products takes place here, as in all decompositions effected between substances that have a very movable constitution, and little disposition to solidity.

The result approximations merely.

In this paper I have attained nothing farther than approximations, but in researches of so difficult a nature, these results are the only ones we can expect. They cannot acquire great precision, but by repeated analyses successively improved.

Note on the Vapour of Ether, § VII, p. 327.

Specific gravity of elastic vapours.

In a paper read to the Society of Natural History and Philosophy at Geneva, December, 1804, I gave the particulars of an experiment made for the purpose of ascertaining directly the specific gravity of the elastic vapour of ether in vacuo. The conclusions drawn by de Laplace from the observations of Watt, my father, and Gay-Lussac, show decidedly, that the elastic vapour of water is found in the same quantity in vacuo and in the air at the same temperature; but we cannot apply the same law to ether, except by analogy, or very indirect experiments. (See the experiments of Mr. Dalton on the evaporation of ether. Manchester Memoirs, vol. V, or our Journal, vol. VI, p. 266.)

Experiment to ascertain that of ether.

I procured a matras, the body of which contained 30 cubic inches, and the cylindrical neck of which was 32 inches long, and about three lines in diameter. On this neck I measured off a length of about two inches, and weighed the quantity of ether requisite to fill this length.

The

The matras was filled with mercury, except a space equal to that of the small column, that had been measured, and this was filled with ether. I then closed for an instant the orifice of the matras, and inverted it in a basin of mercury, under which I opened it. Thus the matras became a kind of imperfect barometer, terminating above in a hollow ball void of air, but filled with the vapour of ether. The length of the column of ether, previously measured, was diminished above a third by the formation of the vapour I have mentioned. This diminution reduced to weight, and compared with the capacity of the body of the matras, gave me the bulk and weight of the vapour of ether in vacuo, and proved, that they were equal, at least as far as could be expected in an experiment on 30 cubic inches, to the bulk and weight of this vapour in atmospheric air, in nitrogen gas, and in hidrogen gas. The vapour of alcohol is too light, to afford sufficiently decided results by this process.

Necessary precautions.

In this experiment there are some precautions to be taken, in which however there is no difficulty, 1st, to expel from the surface of the mercury contained in the barometer a small quantity of liquid ether, which lodges between the mercury and the inside of the neck when the matras is inverted. This may be effected by surrounding it with a cloth warm enough to reduce this ether into elastic vapour.—2dly, it is necessary to estimate by a comparative experiment, made at the same time and in the same place, with a matras of equal size, the weight of the liquid ether that adheres in small quantity to the inside of the body filled with vapour. 3dly, in stopping the matras to invert it, the stopple must not touch the ether. I avoided this source of errour, by fixing in the neck of the matras, near its orifice, a tube closed at the bottom, and filled with the ether intended for this experiment.

2oz of ether fill the space of a cubic foot.

I found thus, that a cubic foot void of air, or filled with air, could contain, at a temperature of 18° R. [72·5° F.], about two ounces of invisible ether in the state of gas. The extraordinary weight of this vapour instructs us how much ether is lost, by employing large vessels or globes passing one into another, for the purpose of condensers and receivers in distilling this fluid.

A know-

Specific gravity of vapours affords useful data.

A knowledge of the specific gravity of vapours may furnish considerable resources in chemical analyses. By the help of this datum, and detonating a few cubic inches of the vapour of ether with oxigen gas, I was able to determine with more precision the proportions of oxigen, hidrogen, and carbon in ether, than by burning two ounces of this liquor in a red hot tube. I obtained results nearly as accurate with the vapour of alcohol.

Vapour of ether employed to ascertain its affinities to pitch,

The vapour of ether may be employed with as little expense for determining the affinities of this fluid to different substances. I introduced over mercury 12 grains of pounded pitch into 20 measures of atmospheric air dilated by the vapour of ether, which consisted of 10 measures of air previous to its dilatation. The 20 measures occupied a column 6 inches high, and 6 lines in diameter; and were reduced to eleven measures by the presence of the dry pitch, which became semifluid in thus condensing almost the whole of the ethereous vapour.

suet,

I obtained a somewhat less condensation with 12 grains of suet, 20 measures being reduced only to 13. The suet was softened.

India rubber,

Twelve grains of caoutchouc, very minutely divided, reduced 20 measures to 15.

camphor,

Twelve grains of camphor reduced 20 measures to 16. The camphor was moistened.

wax,

Twelve grains of yellow wax reduced 20 measures to $16\frac{1}{2}$.

lac,

The vapour had very little action on gum lac, 12 grains of this only reducing 20 measures to 19.

and tragacanth.

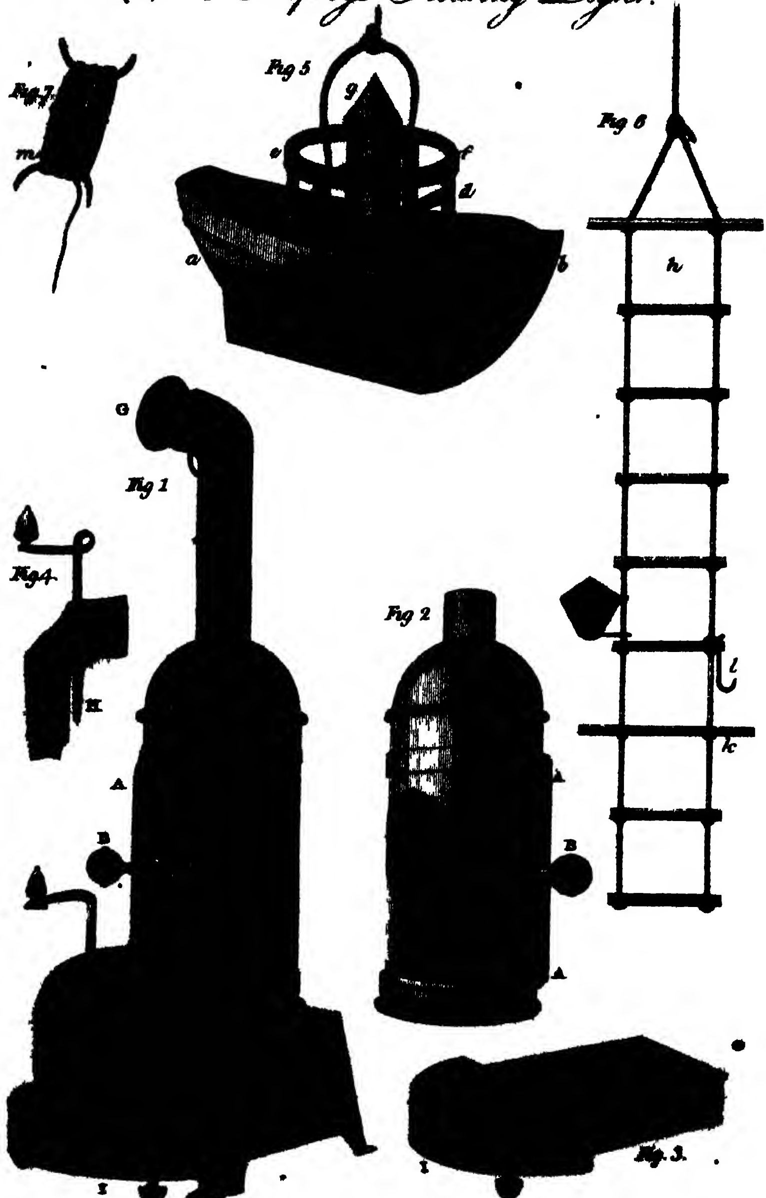
A similar quantity of gum tragacanth produced a condensation too small to be measured.

Specific gravity of vapours in the direct ratio of the volatility of the substances producing them.

The knowledge of the specific gravities of the vapour of water, of alcohol, and of ether, may give us an idea of the law, which the gravities of vapours follow in proportion to the volatility of the liquids from which they arise. Water at a given temperature is less evaporable than alcohol, and alcohol than ether. The elastic vapour of water is lighter than that of alcohol; and the vapour of alcohol is lighter than that of ether. The specific gravity of elastic vapours

then,

Mr. Wm. Shipley's Floating Light.



Mr. Joseph Collier's Ship's Stove.

then, at equal temperatures, appears to be in the ratio of the volatility of the liquors that furnish them. The most volatile bodies are those, which, in similar circumstances, produce the heaviest elastic vapours.

Observations made by several natural philosophers indicate, that gasses of different natures mix uniformly, and do not arrange themselves according to the natural order of their different specific gravities: but if this observation were unfounded, if the vapours that emanate from the terrestrial globe arranged themselves in the order of their gravities, those that belong to the least volatile bodies, as the earths and metals, would be those that would occupy the highest strata of our atmosphere, supposing its temperature uniform.

Gasses mix uniformly without regard to specific gravity.

II.

Description of an improved Ship's Stove, by Mr. Joseph Collier, *No.* 11, *Crown Street, Soho* *.

SIR,

I HEREWITH send you a model of an improved ship stove, which may also be employed in drying houses, &c., with more safety than those in present use.

I submit it to the inspection of the members of the Society, who, I make no doubt, will see its advantages, and am, Sir,

Your humble Servant,
JOSEPH COLLIER.

P. S. The expense of one twelve inches diameter will be about eight pounds.

Fig. 1, *Plate* IX, represents the stove, with the front partly closed by the circular slide *A*, which is moved from the back by the brass handle *B*. *C* a movable plate attached to the slide *A*, now supported by the latch catching a pin,

Description of a ship's stove.

* Transactions of the Society of Arts, vol. xxv, p. 93. Fifteen guineas were voted to Mr. Collier for this invention.

by which means it acts as a blower to cause the fire to burn more briskly, but which slides down also to shut the fire up. *D* another plate, now hanging on its latch, but which can be let down to shut up the ash pit or dish *I*, which can be drawn out when the side facings *FF* are pulled up. *G* a circular plate or cap, which slides so as to shut the chimney up close.

Fig. 2, The body of the stove with the slider *A* moved round to the back, and thus leaving the fire-place completely open.

Fig. 3, The ash-dish shown separate.

Fig. 4, One of the side facings taken out to show the figure *H*, which slides into a hole made in the corner of the stove to hold it.

III.

Account of a Floating Light calculated to save the Lives of Persons, who have the Misfortune to fall overboard in the Night from any Ship. Invented by Mr. WM. SHIPLEY, *Founder of the Society for the Encouragement of Arts, Manufactures, and Commerce* *.

The machine described.

THIS floating light consists of a hollow vessel in the form of a boat, made of tinned iron plate, *a b*, *Fig.* 5, *Plate* IX, the joints of which are carefully soldered, so as to keep out the water. The boat is 27 inches long, 13 broad in the middle, and 12 deep, and is sufficient to support a man in the water. From the gunwale of the boat, on each side, projects a handle *c d*, soldered fast to it for the man to hold by.

ef is a metal ring connected with the boat by four upright pieces, within which is another smaller ring, turning on pivots, fastened to the ring *ef*, in the direction of the boat's length; the internal ring supports a small lantern, *g*, by an axis which passes through it, and is pivoted into the ring at each end, in the direction of the boat's breadth. By

* Transactions of the Society of Arts, vol. xxv, p. 94.

means

means of these rings the lantern will remain in a vertical position, independent of the boat's motion.

Its application.

On the first alarm of a man falling overboard in the night, the candle is to be lighted, and the machine lowered into the sea by the rope; if the man should be at a small distance from the ship, he may, by means of the rope, be taken on board immediately on his reaching the machine, if not, the rope may be secured on the iron reel, to prevent its unwinding, and cast off, and the light will direct the man where to find it, and holding fast by the two handles it will support him in the water.

Fig. 6, *h*, is a rope ladder, having a lantern attached to it, as well to direct the person in the water to the rope ladder, as to enable the persons who lower the ladder to let it down till the cross-bar *k* reaches the water; *l* is a hook to hang the floating light upon. *Fig.* 7, *m*, is the reel for the line, by which the floating-light is to be lowered.

The lamp always to be ready for lighting.

It is proposed, in order to make this float useful, that it be placed every night under the care of the officers on watch; that its lamp be frequently trimmed and supplied with fresh oil, and its wick moistened with oil of turpentine, in order that it may take fire with the least touch of a lamp or candle; and whenever the alarm is given of any of the sailors falling overboard in the night, the officer on watch may light the lamp in the lantern belonging to the float as expeditiously as possible, and let the float down by a small cord, wound upon an iron reel, into the water, till it has floated about one second of time, and the float is a little way out of the perpendicular of the small cord. He is then to secure the cord on the reel, to prevent its unwinding, and toss it overboard. The reel will sink down, and pull the line almost perpendicular, and thus it will not be liable to entangle the person when he swims to the float, who, when he has got hold of the handles of it, may move it very fast which way he will, only by striking his legs in the same manner as he does when he swims; and as the light of the lamp will be a certain guide for the person fallen overboard to find the float, so it will also direct them in the ship to find the man and float: And when the ship has tacked about,

Directions for its use.

and is come to the float, then the following method is proposed to take up the man and float into the ship: viz. A lantern, with a rope ladder, may be let down by a cord from the ship, till a cross-bar below the lantern touches the water, which may be seen by them in the ship by means of the light from the bottom of the lantern; and thus the man in the water may lay hold of the cross-bar, and fix his feet on one of the steps of the rope ladder, and he may then lay hold of the iron bar or handle of the float with one hand, and hang it on the hook of the rope, above the cross bar; which being done, the man and float may be both safely lifted into the ship.

This ingenious and humane contrivance was presented to the Society by Mr. Shipley in 1776, and the silver medal, with a letter of thanks, was voted to him. The machine has been preserved in their repository, but as they consider it to be not sufficiently known, they have published the preceding account in their Transactions for last year. I remember observing, in the time of the American war, that several of our ships kept a small hull of a vessel lashed to the rails of their stern gallery, or their tafferel, ready to cut away the moment a man fell overboard. This hull had a single mast, with a red flag, that the waves might not conceal it from the sight of the man in the water; and was of course much preferable to the common resource, a hen-coop, or a grating. Such a flag might very easily be added to Mr. Shipley's floating light, for use in the day.

Ready contrivance to keep a person afloat that cannot swim.

While on this subject it may not be amiss to notice the contrivance, I believe of the late admiral Locker, by means of which a person who cannot swim may assist another in danger of drowning, and at least keep him afloat, till farther help can be obtained. If a man tie up his hat in a handkerchief, with the knots meeting in the centre of the opening of the crown, he may go into the water safely to assist another, holding the knots in one hand so as to keep the hat upright; for the air in the crown of the hat, while held in this position, will be sufficient to keep two persons from sinking.

IV. *An*

IV.

An Essay on the Sugar of Grapes; By Professor Proust.

Concluded from page 316.

Concluded from page 316.

After observing, that sugar is become an indispensable article of consumption, Professor Proust expatiates on the necessity of finding a substitute for that of the West Indies, should their intercourse with Spain, France, and other continental countries be cut off; and for this purpose he recommends the sugar from grapes. This he confesses is not precisely the same with that of the cane, but may very well supply its place. Without being refined it will answer every purpose, in which colour is no object, as for sweetening coffee, chocolate, or dishes made of milk, in pharmaceutical preparations, &c. If the importation of sugar were stopped, that of the grape might be substituted

When refined, says Professor Proust, it is perfectly white, but will not acquire the solidity of that of the cane, on account of its granular and porous crystallization; so that it cannot be made into loaf sugar, unless the art of the sugar-baker furnish him with resources, which I have no room to expect from the trials I have made. Its qualities.

Its sweetness is evidently inferior to that of the sugar from the cane, so that it must be used in larger quantity; and it is not so readily soluble. It dissolves entirely in spirit of wine; but it separates from it much sooner than that of the cane, and always in tuberculous, granular crystals, in which no determinate arrangement of parts can be perceived.

Presuming, that a comparison of the juice of green grapes with that of the perfectly ripe fruit will not be uninteresting, I shall first give a sketch of the results I obtained by analysing it. In it are found, 1, tartar; 2, sulphate of potash; 3, sulphate of lime; 4, citric acid in abundance; 5, malic acid a very little; 6, extractive matter; and, 7, water. Contents of the unripe juice.

The citric acid is the chief base of this juice. It contains neither gum nor saccharine matter: and in those years when the dearness of lemons does not allow us to extract their Would furnish acid of lemons.

their acid in Scheele's mode, the juice of unripe grape may be employed for the purpose with more advantage than has been supposed.

This converted into sugar and gum,

But the warmth of the weather promotes the maturity of this juice; the citric acid gradually disappears, so that scarcely any traces of it can be discovered in the ripe grape; and the products that occupy its place are the two species of sugar mixed with a little gum. The elaboration of the juice therefore consists in transforming this acid into gummy and saccharine products, in proportion as the fruit approaches maturity.

by parting with oxigen?

or acquiring carbon?

The elements of the citric acid do not differ from those of sugar and gum, as has been discovered; but, since analysis has found likewise, that it contains oxigen, or the acidifying principle, in more abundance than the nutritious products that assume its place, does this acid, during the ripening, merely lose a part of its oxigen, so as to approach nearer their nature? or does it raise itself to the same point by acquiring a larger proportion of carbon? This admirable metamorphosis passes before our eyes every year, yet nature has covered it with a veil impenetrable to them. To return to the fruit of the ripe grape.

Contents of the ripe juice.

This juice, as it flows from the fruit in the press, contains substances of two kinds, some simply mixed, others in solution. The parts mixed are, first, the fibrous and calcareous pulp, which composes the organization of the berry; and, secondly, a portion of the fecula, which we call glutinous, on account of its resemblance to the animalized substance of cheese termed gluten.

These two substances, if diluted, may be separated by the filtration of the juice, though imperfectly, on account of its viscosity, and their tenaciousness, which choak up the filter. But they may be separated much better by heating the juice to ebullition, because they coagulate, and rise to the surface. When scummed, and strained through flannel, the substances remaining dissolved in the clarified juice are,

1, A portion of fecula; 2, crystallizable sugar; 3, sugar not crystallizable; 4, gum; 5, extractive matter, either white, or tinged red, according to the species of grape.

When

When the juice of grapes is boiled down as far as can be done without danger of altering its qualities, it affords a rob, the quantity of which is proportional to the saccharine quality of the grape, and varies from 18 to 32½ per cent. It is difficult however to avoid some degree of empyreuma, particularly if the juice be acidulous. This alteration diminishes the quality of fermenting in the rob redissolved in water, though without annihilating it, as Beccher had concluded from his experiments. The liquid sugar of the cane too, as Duthrone informs us, is much sooner altered by boiling than the solid sugar. Its rob.

The rob boiled down to a certain point crystallizes in a short time. It congeals into a spongy mass, more or less moistened with a sirup, that has a tendency to drain off. Its crystals, when drained, are a mixture of tartar and crystallizable sugar. It was this product, extracted from the muscat grape of Fuencarral, which, after having undergone a few purifications, led me, instructed as I was by Duthrone's excellent work on sugar, to treat the juice of grapes like that of the sugar-cane, Crystallizes.

As the juice contains acids, that hinder the extraction of the sugar, the first step is to free it from these. After the must has been scummed, and while it is nearly boiling, a lixivium of wood ashes is to be added by little and little, as long as any effervescence takes place. The acids may be known to be saturated by tasting the liquor, which will then have only a saccharine taste. It is then to be boiled down to about half, and left to cool in vats, or even in the copper boilers, for there is no danger of verdigrease as in preparing the rob. While it thus stands, the tartar and citric acid, if there were any, being converted into salts of difficult solution, subside with the excess of the ashes, and the sulphate of lime that was in the juice of the grape. The malic acid, converted into malate of lime, remains in the liquor in consequence of its great solubility. Mode of extracting the sugar.

The must prepared in this way indicates 25° or 26° on the areometer. If it were boiled down beyond this, the subsequent clarification would not be so easy, on account of its thickness. It is then to be beaten up with whites of eggs or bullocks blood, heated, scummed, filtered, and boiled Not to be boiled too much. Clarification.

boiled down to the consistence of a sirup, which may be more or less thick, according to the use for which it is intended. This rob, divested of its principal acids, answers as we see to the first product of the cane, saturated and boiled down to the point at which it takes the name of muscovado.

Must boiled down has a slight acrimony, and in a little time becomes solid.

When the must has been thus prepared, it affords us a coloured sirup, though extracted from white grapes. Its taste is sweet and pleasant; but if as much as a spoonful be swallowed, it affects the throat with that slight impression of acrimony, which is experienced from yellow honey. It condenses in eight, fifteen, or twenty days, mose or less, according to the degree to which it is boiled down, into a yellow, granular mass, of sufficient consistency to be pressed into pots, without flowing out if they be set upside down. The sirup that has not been most boiled is the first to become solid. The sugar of grapes appears to require a certain quantity of water for its crystallization, as it is not found in sirup too much boiled. Hence this is longer before it becomes solid, but then it acquires a consistency more convenient for carriage. Lastly, in this state the muscovado of grapes has the consistency, colour, and appearance of that of the sugar-cane. A vessel that contains but sixteen pounds of water will hold twenty five of this sugar, so that its specific gravity is to that of water rather more than as three to two.

For this it should not be too much boiled.

Sugar of the grape compared with that of the cane.

If muscovado of the grape be compared with that of the sugar-cane, we find, that the latter adds to a slight bitterness a peculiar aroma, the character of which is very striking in rum: while that of the grape has no sensible aroma, being a sugar with a flavour of roasted fruit. This taste, as well as its colour, is owing to the concentrated extractive matter; which has the common property of its genus, that of becoming darker coloured, both by simple exposure to the air, from which it attracts some principle, and by being heated. It is this that gives the muscovado of the grape its orange colour; an effect similar to which is produced in the sugar of the cane, the juice of which is nearly colourless. If the muscovado of the grape be diluted with a quantity of water equal to what it has lost, we obtain

obtain a regenerated must far darker coloured than the fresh: but it is to be observed, that the latter, if a considerable surface of it be exposed to the air, soon acquires a similar tint. These effects are peculiar to the extractive principle, the saccharine and gummy being insusceptible of it. Hence it follows, that the change it experiences from these causes united must extend to muscovado, and communicate to it, as to all roasted fruits, more taste and colour. The following is the proportion of the products discovered in this muscovado by analysis.

Its component parts.

	lb.	oz.
Crystallizable sugar	75	
Fluid sugar	24	7
Gum		5
Malate of lime		4
	100	

A little extract.

The quantity of extractive matter could not be estimated, but it must be very little, since the melasses, notwithstanding its colour, is perfectly transparent.

Proportions of solid and fluid sugars.

To discover the proportion of the two sugars I employed the following means. I set to drain heaps of muscovado, evaporated to such a point as experience had taught me was most favourable for the separation of the sirup, or fluid sugar. The latter, collected and kept some time secured against evaporation, has still let fall pulverulent sugar, and in such a quantity, that, from many experiments of this kind, I am persuaded the crystallizable sugar is more than seven eighths of the muscovado. Notwithstanding this, I have not thought proper to set it down above at more than three fourths; and this I mention, that more confidence than it deserves may not be placed in a process that could not possibly be accurate.

The gum separated by alcohol, and then the malate.

But it is not thus with its other component parts, the gum and the malate. If to a hundred parts of muscovado reduced to the state of a thin sirup alcohol be gradually added, the gum is first deposited. The fluid being decanted off, and more alcohol added, the malate will fall down. As I have frequently repeated this experiment with quantities of sixteen hundred grains, I have reason to believe, that the proportion of these is given pretty accurately in the table.

Separation of the solid sugar from the alcohol imperfect.

If the alcoholic solutions of the muscovado be kept covered with a paper only, the solid sugar will separate from it by crystallization, but never so completely as to be able to calculate the quantity, because the fluid sugar retains a good part. The same thing, as has been seen, takes place with honey thus treated.

Gum of the grape.

The gum of the grape is without taste or colour, and does not differ from what I have found in apples, mulberries, medlars, apricots, plums, &c. It is one of the nutritious products of vegetables, resembling gum arabic.

Malate of lime no injury to the sugar.

The malate of lime, we see, is but in small quantity. If the mixture of an earthy salt in a substance intended for food should be thought an inconvenience by those, who have no idea of the composition of vegetables, I would observe to them, that this salt exists in a great number of fruits, particularly the melon and love-apple; that the sulphate of lime is found in much larger quantity in most of our pulse, in wine, in the waters we most prize at Madrid, in several fruits, in the apple, medlar, quince, potato, &c. without having the least effect on our health.

Sugar of the grape sweetens less than the common.

As a condiment the muscovado of the grape does not sweeten as much as common sugar, on account of the water of crystallization it contains, and the inferior sweetness of its crystallizable sugar. To sweeten a pint of water as much as custom requires, two ounces of the sugar from the cane are sufficient; but two ounces and half of that of the grape are necessary; and with these proportions both the solutions mark the same degree on the areometer.

Contains neither free acid, alkali, nor tannin.

The solution of this muscovado changes neither the infusion of litmus nor solution of isinglass. Muriate of tin precipitates from it the colouring principle, as it does that of the juices of the carrot, melon, grape, sugar-cane, and all fruits.

Its uses.

It is very well adapted to milk, coffee, and chocolate; which it sweetens agreeably, without giving them any particular flavour, that can be disliked, as yellow honey does; and the slight acrimony mentioned in the beginning disappears, because it is only the effect of the extractive matter too much boiled down.

The

The muscovadoes I have examined were extracted from the white grape, called *alvilla*, and the black, called the Arragon grape. The first afforded twenty-six per cent, the second thirty. The latter is not perceptibly higher coloured than the other, as the skin of the grape alone is coloured, if care be taken not to mix with the must the juice extracted by pressing. It will perhaps excite surprise, that the must, after being freed from its acids, affords a quantity of muscovado equal in weight to the rob: but the reason of this is, that the tartar, the only acid that precipitates with the lime, and a few particles of calcareous citrate and sulphate, are found in it but in very small quantity. Of this we may judge by the following result, though we may presume there is a little more in the common grape than in the muscadine. A pound of the latter duly treated with spirit of wine does not afford more than 48 grains of tartar.

Black grapes afforded more than white, and scarcely darker coloured.

Very little tartar in the grape.

It is not the tartarous acid, but the malic, that gives grapes their sharpness: and this too is but in small quantity, since a pound of the juice of the muscadine grape does not afford above 40 or 45 grains of malate of lime. Now if we reckon, that this salt contains one third of its weight of earth, it will follow, that a pound of the fruit does not contain much above 30 grains of acid.

Their acid the malic.

Hence we may conclude, that the juice of the grape freed from its tartar, an effect that may be obtained by simply boiling it down to one third, is already a muscovado little different from that of the cane, which equally contains malic acid, if no lime have been employed in its preparation. As the sugar of the grape approaches so near that of the cane in its qualities, we may understand why the rob of the muscadine, dried and poured on a marble, affords a transparent lozenge, without colour, pleasant to the taste, and appearing like barley sugar: but it has the defect of soon growing moist, as the malic acid and liquid sugar occasion it to deliquesce in a short time.

Grape juice simply boiled down to one third almost wholly sugar.

Transparent lozenge from the rob.

It is remarkable, that the common people have already approached very near the art of making grape sugar, in the preparation of their rob; but the last step, that remained for them to take, required a kind of reflection, for which their education is seldom adapted. At Arganda, near Madrid,

The art of sugar making nearly approached by the common people.

drid, and in other places, to prepare their rob they begin by boiling separately with a certain* quantity of lime the juice of grapes, and that of other fruits they intend to mix with it. Thus, taught by necessity to free them from the acids, that would injure the sweetness of the rob, they employ a process truly chemical, to which theory, so long preceded by practice, cannot refuse its sanction.

The grape muscovado an excellent remedy for scurvy.

The muscovado of the grape will some day no doubt be used for other purposes beside food, when it is known, that in it are united the two vegetable products acknowledged to be best adapted for effectually remedying those diseases that are occasioned by the corruption of the blood, or that impoverishment of the humours called scurvy. The employment of the two kinds of sugar with a particular view to ascertain their effects, particularly freed from all the Galenical farrago that might weaken their powers, may furnish the physician with means of cure better adapted to his views, than those imaginary antiscorbutics, that still continue to usurp the place of efficacious remedies, than those salads of scurvy grass, brook-lime, and water-cresses, the heating acrimony of which could not fail to kindle consumptive fires, if the sick to whom they are prescribed were not protected from these by the dissipation of the qualities of the drugs by our infusions, clarifications, and sirups. Let us hear what Tourlet says, speaking of the scurvy:

General remedies for scurvy

"Fresh vegetables, pure air, aliments that contain most of the mucoso-saccharine principle, always infallibly cure the scurvy. The mucoso-saccharine principle contained in most fresh vegetables, in honey, in sugar, and in various fermentable substances, is of all things best calculated for assimilation, and consequently for the regeneration of the fibrine of the blood.

Animal food not always most nutritious.

"Animalized substances are not always the best fitted for nutrition: on the contrary, those are more so, that require for their animalization a sort of fermentation, which elaborates them, and renders them more capable of being assimilated with the substance of the individual, who uses them as food. Children, for instance, thrive much better on mucous and fermentable substances, than on such as are more animalized. Experience, against which there is

no

no arguing, has incontrovertibly proved, that the use of meat is always pernicious to the scorbutic."

The refining of grape sugar must differ but little, if at all, from that of the muscovado of the cane. Both being composed of two sugars, that require to be separated, nothing is required but to boil down the prepared must to a proper degree of consistency, which every refiner by trade will discover. The muscovado of the grape, brought to this point, will condense within a few days into a cellular granulous mass, the intervals of which will be filled with fluid, the common effect of that attraction, which induces the particles of the two sugars to unite with those of their own kind, and separate into two products. These masses being drained, the result is sugar in its first stage of refinement and sirup. The latter, exhausted by fresh crystallizations retains the malate of lime, gum, and extractive principle. These four substances equally form the melasses of the sugar-cane; but that of the grape has not the same unpleasant flavour. Refining the grape muscovado.

The sugar of the grape however does not crystallize like that of the cane; its grain is pulverulent; and as the masses it yields have little consistency, it appears to me doubtful, whether it can ever be brought to such a degree of hardness as that of the cane: at least it would require management, with which I am unacquainted. Cannot be made loaf sugar.

If the sugar of the grape in this point of view afford us a prospect of an important article of trade, the product of its fermentation promises us no less advantage. Nature has given this muscovado such a tendency to fermentation, that it requires nearly the addition of as much water as it had lost, to produce this effect: and in cold countries, where the warmth necessary to this purpose is deficient, if a little dried wine-lees be added to this regenerated must, its fermentation will be still more active, and then it will proceed as briskly as in temperate climes. Dissolved in water it ferments spontaneously:

* One measure of this muscovado dissolved in three of water forms a liquor of equal density with the juice of the Arragon grape, which indicates 17° on the areometer. This produces four measures of a wine of the colour of that of Malaga, and in which a slight flavour of baked and one part produces 4 of a strong wine.

 fruit

fruit is perceptible. It is as strong as the best wine of la Mancha. As it is extremely intoxicating, certainly neither the beer nor the mead of Russia can be put in competition with it for strength or goodness. The muscovado of the grape therefore may furnish the north with a base adapted to the manufacture of all sorts of wine.

The skins of the black grape colour and improve it.

If the skins of black grapes be added to this, it ferments with equal briskness, and acquires not only their colour, but a portion of their astringent principle, which in moderate quantity improves the taste of all wines, and their quality of keeping.

Valuable therefore in northern countries.

This muscovado imported from the south into the north solves a problem of great importance to cold countries. This is, that with the sugar of the grape wine may in future be made in Siberia as readily as in the kingdom of Valencia. And if this production were considered only as a material for making brandy, what advantage would it afford in the ease and safety of conveyance! Would not beer too be much improved, if its fermentation were promoted by a portion of this muscovado *?

The

Barley contains little soluble matter.

* The meal of barley contains but ten or eleven per cent of products soluble in cold water. These consist in equal parts of gum and mucoso-saccharine matter, rendered acrid by a little extractive, and a few flocks of glutine that separate while boiling.

Its farina.

The farinaceous part consists in two or three and thirty parts of starch, and seven or eight and fifty of a granular insipid substance, which is separable from the starch by washing either in cold or boiling water.

Distilled.

By distillation it yields all the products of starch, with some indications of ammonia. Nitric acid employed without heat extricates from it a very little nitrogen.

Malt contains more soluble matter,

Barley that has been perfectly malted does not yield as before ten or eleven per cent of soluble products, but thirty per cent, though of the same nature.

and less starch.

The farinaceous part consists of seven or eight and fifty parts of starch, and twelve or thirteen of the granular substance. The changes produced in the grain by germination therefore fall on this. The same substance is found in the flour of Indian corn, and constitutes near half its bulk.

Not much sugar in it.

As the gummy part has no share in the fermentation, and is still found in the beer, malted grain contains only about fifteen per cent

The celebrated Glauber asserted in his *Prosperitates Germaniæ*, that, if the rob of grapes were sent to countries, to which nature has denied the vine, they might make their own wines, by adding to this quintessence of wine, as he termed it, the water of which it had been deprived. And he said this might be done in all places, and at all seasons. Glauber said the rob of grapes would make wine.

This idea was certainly ingenious, but he should have confirmed it by experience. He did not; and was openly contradicted by Beccher in terms not very civil, who asserted, that he had tried the experiment in vain for a whole year. Beccher denied this.

In defense of Glauber it may be said, that the sugar in the rob, being more or less affected by the reaction of the tartar and other acids, remains so long inactive, as to lead to a belief of its fermentable property being extinct. Notwithstanding this however, it will ferment, and the period may be accelerated easily by the addition of wine lees. I have even now some wine from such a fermentation, which is very strong, and the boiling down has given it a flavour, that is far from unpleasant. But in some parts of Germany the grape has the double inconvenience of being loaded with tartar, and poor in saccharine matter, since it requires six tuns of must to make one of rob; probably therefore it would not be so much disposed to ferment as in hot countries, in Spain particularly, where the poorest juice of the grape commonly yields a fourth part of sugar and very little acid. It will ferment however, but not readily if too much boiled.

It may not be improper to introduce here the remarks I have had an opportunity of making during the course of a few summers on the fermentation of clarified must.

When the juice of the grape has been clarified by heat and filtration alone, it always continues a little foul, be- Clarified must

cent of saccharine matter. If now we compare barley malt with the muscovado of the grape with respect to their fermentable parts, we shall find, that one hundred weight of the latter nearly equal seven hundred weight of the former. Hence we may judge of the advantage, that would accrue from employing a portion of this muscovado in making beer.

Water heated to 50° does not dissolve starch: this is the reason why the water in brewing is seldom allowed to exceed this point.

cause

cause it retains in solution a portion of the fecula that has been mentioned, and the nature of which has been completely ascertained by Fabbroni and Thenard. This fecula is retained there apparently by the intervention of acids, since we do not find it in the juice, that has been saturated by the carbonate, and clarified with whites of eggs; in which way alone it is obtained perfectly clear.

ferments though freed from fecula.

Fabbroni and Thenard have considered this fecula as a ferment indispensable to the change of the saccharine matter: but when the juice of the grape has been carefully freed from it, the fermentation takes place as briskly as in must not clarified, and we find it pass through all its stages in the same period, without depositing any thing but tartrite of lime.

The cause of the fermentation is in the liquid sugar.

The true cause of fermentation in juices, whether clarified or not, does not reside in this fecula therefore, but in the fluid sugar, the only principle of fruits that is truly fermentable of itself, and capable of imparting this movement to solid sugar. Deyeux appears to me to be the first who observed this difference, and it must be confessed, that all the phenomena of fermentation tend to confirm his opinion. Let us take a rapid view of them.

1st stage of fermentation.

The first effect of fermentation on a juice that has been clarified but not saturated is the absorption of the first portions of carbonic acid, that begins to be evolved. This product occasions the honied sweetness to be succeeded by a brisk taste, which, without being spirituous, renders the must far more pleasant than it was before; and it is in this state, that children like it so much.

2d stage.

The second is the increase of the bulk of the liquor with a temperature exceeding that of the atmosphere, though diminished by all the heat the carbonic acid gas carries off, and the opacity of whey not well clarified.

3d stage.

At the third period the spirit of wine begins to appear, and then the presence of this frees the must from its fecula, and a great part of its tartar. The gum, extractive matter, and malic acid subsist amid the fermentation, without taking the least part in it, since we find them in the same proportions after it is over.

If

If the wine be filtered when at its greatest degree of opacity, its fermentation is perceptibly checked; but it afterward revives, and pursues its course without depositing any thing but particles of fecula and pure tartar. This fecula, or second lees of wine, is always loaded with tartar: but when it has been copiously washed, we find in it all the characters on which Thenard has insisted, and particularly those appearances, that have led Berthollet to compare it with starch. It is perfectly insoluble; grows sour, ferments, and acquires the bad smell of the gluten of wheat; in a word it becomes cheese. When it is dry it is a little transparent, horny, and affords all the products of animalized matters. Potash dissolves it, and separates it from the parts that are purely fibrous. In fine, it is the same thing as the unclarified must rejects in the first moments of fermentation; and if it do not separate from it at the same period, it is because its solubility retains it in the liquor, till the alcohol comes to precipitate it. Other circumstances confirm the fact, that this fecula is no more necessary to the transformation of the two sugars into alcohol, than the former, or than the gum, extractive matter, tartar, &c. If we take must saturated and clarified with whites of eggs, fermentation commences in it the next day. It pursues its course without depositing any fecula, but tartrite of lime alone; and without yielding any thing but carbonic acid.

Filtering checks the fermentation, but it again revives.

The fecula.

In the space of a month the liquor falls from 17° on the areometer to 1° or 2°. If we analyse the residuum after distillation, we shall find again the gum, malic acid, extractive matter, vinegar, some remains of sugar, and nothing more.

The liquor grows lighter. Its residuum.

The muscovado brought to 17° by a sufficient quantity of water ferments completely, changes into wine, and deposits but a few particles of matter. Where then is the influence of the fecula, the tartar, the acids, and the extracts? But the best clarified must will no doubt retain a portion of fecula; and it may be said, that this excites fermentation in the sugar. If this be the case, I would answer, the fermentation should be weaker in proportion to the loss of this principle occasioned by the clarification of the must; but we do not find, that this is at all behind that which

Fecula has nothing to do with fermentation.

which retains the whole of its fecula. Hence let us conclude, that the fecula is one of those products, which are not necessary to fermentation, and that one of the first effects of this change is to free the juices from it, as it frees them from the tartar and sulphate: that, if fermentation required some of the other products of vegetation, to enable it to produce its due effect, it is much more natural to suppose, that those which their solubility renders injurious to the sugar would take a part, than an insoluble substance, which we always find again subsequent as well as previous to it, and of which not the least traces are to be found in wine or its products.

The fecula altered by fermentation.

The fresh fecula of the grape mixed with a solution of sugar at 17° is incapable of fermentation, as Berthollet and Thenard have already observed. I have also ascertained this fact. But if with such a solution of sugar we mix the same fecula after wine has fermented on it, or after it has become lees, it will excite a very brisk fermentation in it in a few hours.

Fecula of the 2d stage.

The white and muddy fecula deposited in the second stage of fermentation does not dissolve in the fermenting liquors; it undergoes no decomposition in them; it changes neither its bulk nor appearance; and there is no trace of it discoverable in the wine. It appears to take no part in the phenomena of fermentation, yet it impresses on crystallizable sugar the fermentative motion. In this case we see clearly, that it acts as matter impregnated with a principle which it transmits. What then is this principle? All that remains for us is to examine, whether we can divest fecula or lees of this impregnation, this leaven, which fits them for exciting fermentation; to enable us afterward to determine, whether the lees themselves really possess this property, or whether they act only by virtue of this principle, in which case they are merely a vehicle. This is a point on which Seguin appears to be occupied.

Does it contain a principle of fermentation.

The gluten not affected by fermentation.

In several spirituous fermentations, in which I have employed yeast, or meal, the gluten has always risen to the top, and adhered in shreds to the mouths of the vessels; and I could easily perceive, that it had neither altered its nature, nor been affected by the changes of the fermenting medium.

I have

Melasses of the grape ferments of itself.

I have said, that the liquid sugar was fermentable *per se*. Melasses from the muscadine grape, separated from its crystallizable sugar, has not lost the property of fermenting. Alone and simply dissolved in water, notwithstanding having been tortured by a number of evaporations, and treatments with chalk and spirit of wine, and its extractive principle having acquired an extremely disagreeable acrimony, it has notwithstanding afforded a strong wine.

The crystallized sugar not tried alone.

I have not yet tried to ferment the crystallizable sugar of the grape, to ascertain whether it be fermentable *per se*: this is a step I mean to take, as soon as I have a sufficient quantity; but I suspect beforehand, that it is not any more than the sugar of the cane.

Tartar not necessary to fermentation

The tartar is a product of vegetable elaboration, like all those that accompany it in the juice of the grape, but it is not a necessary ingredient of fermentation. If nature had intended it to concur in its phenomena, she would not have given it that slight solubility, which occasions its separation in the beginning, when the sugar would have the most need of its influence. Glauber was well convinced of this; and accordingly he recommends the separation of the tartar from the rob, after diluting it in warm water; "for thus," he says, "it will be freed from its acidity, and the wine will be rendered sweeter." It is surprising that Glauber, who had considered the subject so well, did not think of saturating the must.

Bullion asserted, that it was essential.

The experiments, on which Bullion is desirous of establishing the necessity of tartar, have led him to consequences much better calculated to increase the vague ideas respecting fermentation, of which we have already too many, than to elucidate its theory. If the tartar contributed to the alterations of the sugar, we must admit, that the part it acts is purely mechanical, since we find it entire after the formation of the wine. We cannot avoid surprise at the assertion, that must would not ferment without tartar, from one who had daily before his eyes the fermentation of apples, pears, the sugar-cane, services, oranges, gooseberries, cherries, and all kinds of fruit, the juices of which are destitute of tartar; as well as that of honey, of sugar assisted by yeast, and of malted grain. His analyses are not more conclusive. What

This controverted by facts.

too must be the quality of the grapes, the juice of which afforded him but four drachms of sugar to the pint? and this sugar, how could he characterize it as of the same species with that of the cane? Farther, on what grounds can he talk of fermentation, and its produce in spirit, from trials in which we find sugar employed in the proportion of one pound to fifty quarts of water? The liquorice water, that children sell by the shell-full, is not poorer stuff.

Tartar added to the juice of sour grapes will not make wine, If it were true, that fermentation caused the tartar to concur in the production of wine, and even that it could consume fresh quantities for this purpose, as Bullion asserts, we ought never to meet with it in our casks; and the juices most abundant in tartar, those of the years in which the grape does not ripen fully, would afford wines most abundant in brandy.

or increase the produce of spirit. If we could believe, that doubling the quantity of tartar would occasion the produce of spirit to be half as much more, what better use could we make of this salt, than adding it to the must in the proportion of half a drachm to a quart, the dose that he asserts occasioned his obtaining half as much more brandy?

Price of the grape muscovado. With regard to the price at which the muscovado of the grape can be afforded, thirty pounds, under the most unfavourable circumstances, and making full allowance for every thing, cost at Madrid 45 reals [20s. 1¼d.]; but had every thing been bought at the best hand, and the laboratory been a place fitted up for the purpose, the cost would certainly not have exceeded 30 reals [13s. 5¼d]: and in what part of the kingdom of Spain is coarse sugar or even honey to be bought for a real [5d $\frac{1}{8}$] a pound? Add to this the tuns of grapes annually wasted in the country. At Toro, this year, I am told, that the beggars, after being glutted with grapes that they could not consume, left above 170000 arrobes [about 2125 tuns], or about 50000 arrobes [625 tuns] of muscovado, And at Aranda de Duero 2000 cantars [500 galls.] of wine, that could neither be sold nor consumed, were thrown into the kennels; and 150000 were left in the vineyards.

Lime dissolves in spirit. A fact that should not be omitted is the solution of lime in spirit, which I believe has not been observed. I distilled twenty five pints of red wine of la Mancha, adding a handful

ful of quicklime, to obtain at once a product free from the vinegar, which is always found in the first distillation: but the brandy came over so strongly imbued with the smell and taste of the lime, that I was surprised. This spirit in fact contained lime, as was demonstrated by all the tests; and its solution was so far from the effect of some unobserved circumstances, that, when I redistilled it with a gentle heat, it rose again with all its disagreeableness. Even now, after the lapse of three years, the spirit is not altered; it precipitates the metallic solutions, and oxalic acid, and restores the blue colour of litmus reddened by an acid. This solution then is a new point of similitude between the earths and alkalis.

Purification of the grape juice.

I have only to add, that recent trials have taught me nothing more is necessary, to saturate and clarify the juice of the grape, but to throw some powdered chalk into it, agitate the mixture, and let it stand till the next day. The fecula and earth will unite; the juice is then to be strained, boiled, and scummed; and whites of eggs are unnecessary.

The least boiled crystallized the soonest

Desirous of knowing what degree of boiling down was most favourable for the crystallization, I made five experiments in the following order. Having clarified and saturated some juice, I boiled down one portion of it so as to leave but thirty-two hundredth parts of extract; another to thirty-four hundredths; a third to thirty-five; a fourth to thirty-six; and a fifth to forty. Of these the last crystallized first, next that of thirty-six, and then that of thirty-five. Those of thirty-two and thirty-four have not crystallized yet. Hence it is evident, that those sirups, which are least boiled, are the first to yield their sugar.

Soup.

Meat-soup contains fifty per cent of a savory extract, analogous to the product I have obtained from the fermentation of cheese and gluten. This extract is the condiment, the perfume, the quintessence of the soup: is that of bones comparable to it?

V. *Account*

V.

Account of a simple Improvement in the common Still. In a Letter from Mr. J. ACTON.

To Mr. NICHOLSON,

SIR,

Improvement in stills.

I SEND you an outline of an improvement I have added to my common still and worm tub, which I have found of such great utility, that I cannot resist the desire I have of communicating it. The still holds about nine gallons, and is used for distilling common water, essential oils, and water impregnated with them The tub holds about 30 gallons, and not being near any water, I was accustomed to have a great deal of trouble in changing that in the tub when it became hot, which it did very soon after commencing the operation. It was this trouble, that put me to the necessity of contriving the additional condenser, which, though very simple, I have found to answer every purpose I could wish; and I can now distil any length of time without the water in the tub being scarcely raised a degree in temperature, or requiring to be changed, as the heat accumulates in the additional condenser, and when elevated to about 140° or 150°, passses off by evaporation.

This condenser consists of a trough, *a*, *Plate* X, *Fig.* 1, three feet long, twelve inches deep, and fifteen inches wide; with a pewter pipe, *b*, passing through the middle of it, of about two inches diameter at the largest end, and gradually tapering to about three quarters of an inch at the smaller end. It is most likely, that so simple and so useful a contrivance must have been thought of before, but never having seen or heard of any, I have taken the liberty of troubling you with it, requesting you will exercise your own judgment as to the propriety of inserting it in your Journal, as I have so firm a reliance upon the justice of it I cannot be otherwise than pleased with your decision.

I remain, Dear Sir,

Your obliged and faithful Servant,

J. ACTON.

Ipswich,
August 19, 1808.

VI. *Description*

Numerical Carbonate of Lime.

Fig. 3

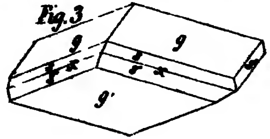


Fig. 2.

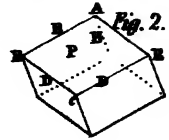
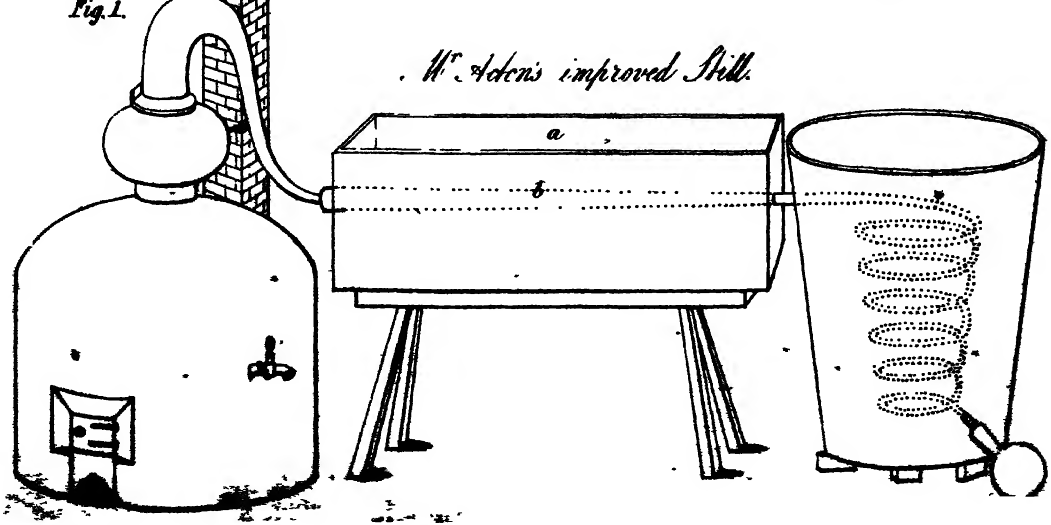


Fig. 1.

Mr. Adcns improved Still.



VI.

Description of a new Variety of Carbonate of Lime. By R. J. Haüy *.

In my Treatise on Mineralogy I described forty-seven determinate varieties of form in carbonate of lime. About two years after I published in the Annals of the Museum of Natural History a memoir containing a description of thirteen more varieties of the same substance, making together sixty; and since that time I have observed eleven, equally marked with novelty, so that at present the number of forms presented by this species amounts to seventy one.

71 varieties of carbonate of lime known.

This number is trifling to what theory demonstrates to be possible, which exceeds eight millions, even supposing we confine ourselves to the four simplest laws of decrement. But I am far from considering the formula, that has led me to this result, as exhibiting the sum of past and future discoveries; and we need not fear being perplexed at some future period by our abundance, if we consider, that among the circumstances capable of determining the production of this immense quantity of crystalline forms there are a great number, that do not exist in nature. The formula to which I allude merely shows us how fertile the laws of the structure of crystals are in themselves; and teaches us, that Science has in her hands certain means of determining with precision all the new forms, that may present themselves to mineralogists in the course of their researches, however varied they may be, and however little analogy they may bear in appearance to those that are already known.

Above 8 millions possible,

but circumstances requisite to the production of all do not exist.

The very steps that Science takes in her progress, in proportion as she enriches herself by discovery, indicate, that what does exist is confined to very narrow limits compared with what is capable of existing. The new varieties of carbonate of lime, that have been found within these few years, are almost all of them but different combinations of laws already observed, and the greatest of these combinations does not include above six quantities.

Only a small part of things possible actually exist.

* Journal des Mines, vol. XVIII, p. 299.

In

Only two laws to add to those before known.

In the applications I have made of the theory to these varieties I have found but two laws to add to the twenty-one I had mentioned in my treatise as those, of which I had then recognised the existence. The first determines in part the structure of a variety, which I have called trihexaedral carbonate of lime, because its figure is that of a six-sided prism, terminated by right pyramids of the same number of faces. Three of these faces are parallel to those of the primitive rhomboid, and the other three, which have the same inclination, result from a decrement by two rows in height on the inferior angles *e*, *Fig.* 2, *Plate* X, of the same rhomboid, so that if this law attained its limits, the secondary form resulting from it would be similar to the nucleus. This structure is likewise that of the prismatic hyaline quartz, which I have described in my Treatise on Mineralogy, vol. ii, p. 411; but in the quartz crystals the inclination of the terminal faces to the adjacent sides is 141° 40′, while in the carbonate of lime it is only 135°; which arises from the difference that exists between the primitive forms themselves. I am indebted to Mr. Héricart-Thuri, mine-engineer, for the knowledge of this interesting variety, a specimen of which he has presented to me.

Trihexaedral carbonate of lime.

Prismatic hyaline quartz.

New variety of carbonate of lime.

The second law relates to the variety, that forms the subject of this article. The crystals, that have enabled me to determine it, were sent me from Clermont-Ferrant by Mr. Augustus Mabru, whose useful researches in the department of Puy-de-Dôme, as well as those of his worthy friend Mr. de Laizer, afford new proofs of the mineralogical treasures contained in that country. I avail myself of this opportunity, to render them both a public testimony of my gratitude for their eagerness to impart to me the fruits of their discoveries, particularly with respect to the species sulphate of barytes, of which they have sent me a considerable number of varieties hitherto unknown; and this pleasure has been greatly enhanced, as the symbols representing the laws of their structure, given in the letters accompanying them, generally announce observers equally attentive and enlightened.

Its figure,

Fig. 3 represents the variety in question. Mr. Mabru had very justly remarked, that it exhibited a rhomboid with equal

equal axes, each of the six lower edges of which was replaced by a bevel with two facets, s, s''. Hence it follows, that, if we suppose these facets prolonged till they meet, so as to conceal the faces g, g, of the equiaxal rhomboid, the crystal would be a dodecaedron with scalene triangles, analogous to that of the metastatic variety, commonly called dog-tooth spar; and if we were farther to imagine planes passing through the edges x, x, &c., these planes would intercept a rhomboid similar to the equiaxal, and which with respect to the dodecaedron would have the same position, as the primitive rhomboid has with respect to the metastatic dodecaedron; so that the equiaxal rhomboid may be considered as a hypothetical nucleus with respect to the dodecaedron before us. We have already an instance of a hypothetical nucleus of the same kind in the paradoxal carbonate of lime discovered by the learned Mr. Tonnellier, keeper of the mineralogical collections of the Council of Mines. But in the latter variety the hypothetical nucleus is the inverse rhomboid; and it is remarkable, that the forms hitherto exhibited by these sorts of hypotheses are engrafted, as it were, on the two secondary rhomboids, the most simple among those that belong to carbonate of lime.

It was easy to see at once, that the facets s, s'', must depend on a law intermediate to the angles $E\,E$ of the nucleus, *Fig.* 2, which is likewise the case with the paradoxal variety. Now in this there are two lines of particles subtracted from the edges $D\,D$, and only one from the edges $B\,B$, which is the most simple of combinations of this kind: and if we add the condition, that the hypothetical nucleus is the inverse rhomboid, it necessarily follows, that the intermediate decrement takes place with a single row. In the variety discovered by Mr. Mabru the two terms of combination are greater by unity than in the preceding, that is, there are three lines of particles subtracted from the edges $D\,D$, and two from the edges $B\,B$; and combining these data with the condition, that the hypothetical nucleus is the equiaxal rhomboid, we find, that the intermediate decrement is at the same time mixed, and takes place by five rows in breadth and six in height. Any other law would give for the nucleus a rhomboid different from the equiaxal. For instance, if

and structure.

if we suppose the intermediate decrement to be made by a single row, the hypothetical nucleus would become a rhomboid extremely flattened, in which the great angle of the rhombus would be 119° instead of 114° 18′, and the greatest angle of incidence between the faces 160° 26′ instead of 134° 23′: and besides, on this supposition the values of the angles of the dodecaedron would differ very sensibly from those that give the law of $\frac{5}{6}$, which agree with observation.

Measures of its angles.

The following are the measures of the saliant angles. Between g and g, 134° 23′ 38″; s and s'', 118° 29′ 4″; s and g, or s'' and g', 143° 32′ 39″; s and s, 115° 1′ 44″; s and s', 142° 24′ 6″.

Numerical carbonate of lime.

I give this variety the name of numerical carbonate of lime, on account of the properties of the numbers that express its form; the sum of the exponent of B, which is 2, and of the exponent of D, which is 3, being equal to the numerator of the exponent of E, which is 5, and their product being equal to its denominator, 6.

I have likewise investigated the law that would govern the dodecaedron, if the hypothetical nucleus were substituted for the true; and I have found, that in this case the symbol of the dodecaedron would be $\overset{\frac{5}{3}}{D}$, a quantity the exponent of which is double that of E in the preceding case.

Possibility of substituting a secondary form to the primitive one.

In my Treatise on Mineralogy, vol. ii, p. 15 and following, I have developed the theory respecting the possibility of thus substituting a secondary form to the primitive one, so as to derive any other secondary form from it by the laws of decrement. This view gives an infinite scope, if I may be allowed the expression, to the results of that branch of geometry, which arises from the study of the laws, to which the wisdom and power of the supreme Being has subjected the formation of the regular bodies, that people the subterranean world; and our admiration increases, when we see this immensity of results end in one common term, the invariable form of the particle shown by the dissection of crystals. I ought to say here in particular, that none of the varieties of carbonate of lime exhibit the rhomboid of 101° 30′ by the help of mechanical division with more facility, and more neatly, than that which has been described

This variety easily dissected.

Mr.

Mr. Mabru found this variety at the foot of Puy-Saint-Romain, below the plaster quarries of St. Maurice, about ten miles south-east of Clermont, in the department of Puy-de-Dôme. Its gangue is a compact carbonate of lime of a gray colour, mixed with a little argile and oxide of iron. The largest crystals I have observed, are about 18 mill., or 8 lines [7 lines Eng.] broad. In the same place is found equiaxal carbonate of lime without any modification.

Where found.

Size.

VII.

Second Letter on the Subject of the New Metals. By Mr. A. Combes.

To Mr. NICHOLSON.

SIR,

As you have candidly admitted a letter into your Journal, in which your own statements (as it seems) are censured, you will not, I trust, refuse a place to my reply to your observations *. It may be considered as presumption in an obscure individual, to enter into the lists with a veteran in science; and this would be the case, were the question of any other nature than merely concerning historical documents; upon these topics the mere man of leisure may have an advantage over the man of business and genius; and to refer to authorities requires no great intellectual exertion.

The question respects historical documents

I still maintain, in opposition to your opinion, for which on all other occasions I have the highest respect, that the true *alkalis* " were never long ago suspected to be metallic oxides."

Fourcroy's testimony equivocal.

You mention the testimony of Fourcroy, but the passage to which you refer is undoubtedly equivocal.

Mr Fourcroy says, Systeme des Con. Chem. II, pag. 196, " l'opinion sur la pretendue nature metallique de la Barite ainsi que celle des autres bases salifiables surtout terreuses ne sera qu'une hypothese." Here " des autres" ought,

* See Journal, p 231—4.

a strict grammatical propriety to be translated "others," and not the other. And Fourcroy throughout his work never hints at any suspicion of soda, potash, and ammonia being metallic.

Mr Kerr's not a guess

You have quoted Mr. Kerr, but what he has said is not a suspicion or guess, but a statement of facts, which for many years have been known to be false, and which he has never condescended to cor . Magnesia, according to him, had been proved by Tondi and Ruprecht to be a *metallic oxide*. Soda, he says, appears from some experiments published in the Turin Transactions, to be a modification of magnesia, therefore soda must be also a metallic substance. Now here are no analogies brought forward, nothing which can be called an hypothesis, but a mere plain downright statement of an errour.

Tondi and Ruprecht's experiments.

I am a little surprised at the view which you yourself have taken of Tondi's and Ruprecht's experiments. You state, that alkali was certainly present, but that the alloys were like phosphurets of iron. You do not refer to what the sagacious Klaproth has said after a minute examination of these results, *Annales de Chimie*, IX, page 287. "The pretended reduction of earths into metals, is nothing but a pure illusion;" nor do you notice, that Savaresi, by a most elaborate and elegant series of experiments, proved, "that they could be produced or not at pleasure, not in consequence of the presence or absence of alkali, or alkaline earths, but in consequence of the presence or absence of bone ashes." *Annales de Chimie*, IX, page 156.

Mr Kerr

You certainly may find one authority, showing that almost every "thing is metallic," deduced from the very book which you have already quoted;" for Mr. Kerr is disposed to place charcoal, phosphorus, and sulphur, amongst the metals, for the very reasons why they ought to be excluded from this class of bodies. He says, "why should carbon, sulphur, and phosphorus, not be considered as metals, because their specific gravity, lustre, and ductility, differ from the bodies called metals, which differ so much in these respects amongst themselves?"

Even the

There are no persons in general more ready to lay claim to

to the foundations of a discovery, if not to the discovery itself, than our neighbours on the continent, yet on this occasion they have been anticipated at home: for in a report of the Polytechnic School, published in the last Number of the Phil. Magazine, it is said by the editors of the journal of the Polytechnic School, "that Mr. Gay Lussac, and Mr. Thenard, had repeated Mr. Davy's experiments, and obtained the two new metals, of which the existence had not been suspected previous to Mr. Davy's experiments."

French admit Mr. Davy's claim to the discovery.

You say, it is no derogation to Mr. Davy's merits, that he has explored the processes of nature by simplicity of investigation, and clear deductions grounded upon a knowledge of the antecedent analogies. On the last part of this proposition I cannot agree with you. It would in my opinion have been a derogation to his merit, had he been guided by any analogies so loose as those, which might have led him to look for metals in the fixed alkalis. He was on the contrary enlightened by new principles of research, arising from the knowledge of the properties of chemical decomposition by Voltaic electricity, which your useful labours partly led the way to, and which his discoveries have made almost universal.

Mr. Davy's discovery.

I attended his course of lectures of 1807, and in referring to my notes I find, that he stated it as a fact, that all bodies of known composition attracted by the negative pole in the Voltaic circuit consisted principally of inflammable matter, and were naturally positive; and that it was probable therefore, that all bodies of unknown composition attracted by this pole, and which were naturally positive, might also contain inflammable matter.

The negative pole attracts only inflammable matter.

In his lectures in 1801, he stated, that, in looking for *inflammable matter* after those ideas in the fixed alkalis, he had *discovered* it, and that he had likewise found what he had not expected, that it was metallic in its nature.

This he looks for in alkalis.

In this instance sagacious conjecture and sound analogy were followed up by experimental research, and ended in a great discovery.

Guesses, except from experimental inquirers, ought scarcely to be tolerated in science; and to attach importance to them, and to dignify them with approbation, is merely

Guesses in science

merely to encourage a waste of time and a tendency to dreaming. Mere barren hypothesis, that neither arise from facts, nor lead to experiments, are *weeds* in the field of science which will always grow sufficiently without manure.

You, as an experimental poilosopher and a lover of truth, ought to endeavour to check their growth; and should your Journal be made a hotbed for their cultivation, it must inevitably loose its ancient universally acknowledged utility and importance.

I am, Sir,

Your obedient humble Servant,

A. COMBES.

Chelsea,
November 17, 1808.

VIII.

Electro-Chemical Researches, on the Decomposition of the Earths; with Observations on the Metals obtained from the alkaline Earths, and on the Amalgam procured from Ammonia. By HUMPHRY DAVY, *Esq. Sec. R. S. M. R. I. A.* *

1. *Introduction.*

IN the Philosophical Transactions for 1807, Part I †, and 1808, Part I, I have detailed the general methods of decomposition by electricity, and stated various new facts obtained in consequence of the application of them.

Decomposition of the fixed alkalis led to hopes of similar results.

The results of the experiments on potash and soda, as I stated in my last communication to the Society, afforded me the strongest hopes of being able to effect the decomposition both of the alkaline and common earths; and the phenomena obtained in the first imperfect trials made upon these bodies countenanced the ideas, that had obtained from the

* Philosophical Transactions for 1808, Part II, p. 333.
† See Journal, vol. XVIII, p. 321, and XIX, p. 37.
‡ Ibid. Vol. XX, p. 290, 321.

earliest

earliest periods of chemistry, of their being metallic in their nature*.

Many difficulties however occurred in the way of obtaining complete evidence on this subject: and the pursuit of the inquiry has required much labour, and a considerable devotion of time, and has demanded more refined and complicated processes, than those which had succeeded with the fixed alkalis. Many difficulties occurred.

* Beccher is the first chemist, as far as my reading informs me, who distinctly pointed out the relations of metals to earthy substances, see Phys. subt. Lipsiæ, 4to, p. 61. He was followed by Stahl, who has given the doctrine a more perfect form. Beccher's idea was that of a universal elementary earth, which, by uniting to an inflammable earth, produced all the metals, and under other modifications formed stones. Stahl admitted distinct earths, which he supposed might be converted into metals by combining with phlogiston; see Stahl Fundament. Chym. p. 9, 4to, and Conspect. Chem. 1, 77, 4to.—Neuman gives an account of an elaborate series of unsuccessful experiments which he made to obtain a metal from quicklime. Lewis's Neuman's Chem. Works, 2d. edit, vol. i, p. 15. The earlier English chemical philosophers seem to have adopted the opinion of the possibility of the production of metals from common earthy substances; see Boyle, vol. i, 4to, p. 564, and Grew, Anatomy of Plants, lec. ii, p. 242. But these notions were founded upon a kind of alchemical hypothesis of a general power in nature of transmuting one species of matter into another. Towards the end of the last century the doctrine was advanced in a more philosophical form; Bergman suspected barytes to be a metallic calx, Præf. Sciagrap. Reg. Min. and Opusc. iv, 212. Baron supported the idea of the probability of alumine being a metallic substance, see Annales de Chemie, vol. x, p. 257.—Lavoisier extended these notions, by supposing the other earths metallic oxides. Elements, 2d edit. Kerr's translation, p. 217. The general inquiry was closed by the assertion of Tondi and Ruprecht, that the earths might be reduced by charcoal; and the accurate researches of Klaproth and Savaresi, who proved by the most decisive experiments, that the metals taken for the bases of the earths were phosphurets of iron, obtained from the bone ashes and other materials employed in the experiment, Annales de Chemie, vol. viii, p. 18, and vol. x, p. 257, 275. Amidst all these hypotheses, potash and soda were never considered as metallic in their nature; Lavoisier supposed them to contain azote; nor at that time were there any analogies, to lead that acute philosopher to a happier conjecture. Early notions of the metallic nature of earths.

From the infusibility of the earths.

The earths like the fixed alkalis are nonconductors of electricity; but the fixed alkalis become conducting by fusion: the infusible nature of the earths, however, rendered it impossible to operate upon them in this state: the strong affinity of their bases for oxigen, made it unavailing, to act upon them in solution in water; and the only methods, that proved successful, were those of operating upon them by electricity in some of their combinations, or of combining them at the moment of their decomposition by electricity in metallic alloys, so as to obtain evidences of their nature and properties.

A more powerful apparatus wanted.

I delayed for some time laying an account of many of the principal results which I obtained before the Society, in the hopes of being able to render them more distinct and satisfactory; but finding that for this end a more powerful battery, and more perfect apparatus than I have a prospect of seeing very soon constructed, will be required, I have ventured to bring forwards the investigation in its present imperfect state; and I shall prefer the imputation of having published unfinished labours, to that of having concealed any new facts from the scientific world, which may tend to assist the progress of chemical knowledge.

2. Methods employed for decomposing the alkaline Earths.

The alkaline earths moistened, and electrified under naphtha. Inflammable gas evolved, and metallic points appeared.

Barytes, strontites, and lime, slightly moistened, were electrified by iron wires under naphtha, by the same methods, and with the same powers as those employed for the decomposition* of the fixed alkalis. In these cases, gas was copiously evolved, which was inflammable; and the earths, where in contact with the negative metallic wires, became dark coloured, and exhibited small points having a metallic lustre, which, when exposed to air, gradually became white; they became white likewise when plunged under water, and when examined in this experiment by a magnifier, a greenish powder seemed to separate from them, and small globules of gas were disengaged.

In these cases there was great reason to believe, that the earths had been decomposed; and that their bases had com-

* See page 4, or Journal, vol. xx, p. 291.

bined

bined with the iron, so as to form alloys decomposible by the oxigen of air or water; but the indistinctness of the effect, and the complicated circumstances required for it, were such as to compel me to form other plans of operation.

The strong attraction of potassium for oxigen induced me to try whether this body might not detach the oxigen from the earths, in the same manner as charcoal decomposes the common metallic oxides. Potassium tried to attract the oxigen from the earths.

I heated potassium in contact with dry pure lime, barytes, strontites, and magnesia, in tubes of plate glass; but as I was obliged to use very small quantities, and as I could not raise the heat to ignition without fusing the glass, I obtained in this way no good results. The potassium appeared to act upon the earths and on the glass, and dark brown substances were obtained, which evolved gas from water; but no distinct metallic globules could be procured: from these circumstances, and other like circumstances, it seemed probable, that though potassium may partially deoxigenate the earths, yet its affinity for oxigen, at least at the temperature which I employed, is not sufficient to effect their decomposition. Ineffectual.

I made mixtures of dry potash in excess and dry barytes, lime, strontites, and magnesia, brought them into fusion, and acted upon them in the voltaic circuit in the same manner as that I employed for obtaining the metals of the alkalis. My hopes were, that the potassium and the metals of the earths might be deoxigenated at the same time, and enter into combination in alloy. Potash and the earths tried to form an alloy.

In this way of operating, the results were more distinct than in the last: metallic substances appeared, less fusible than potassium, which burnt the instant after they had formed, and which by burning produced a mixture of potash and the earth employed; I endeavoured to form them under naphtha, but without much success. To produce the result at all required a charge by the action of nitric acid, which the state of the batteries did not permit me often to employ*; and the metal was generated only in very The results rather more distinct.

* The power of this combination, though it consisted of one hundred plates of copper and zinc of six inches, and one hundred and The Voltaic battery weakened by use.

very minute films, which could not be detached by fusion, and which were instantly destroyed by exposure to air.

As potash mixed with metallic oxides was rapidly decomposed,

I had found in my researches upon potassium, that when a mixture of potash and the oxide of mercury, tin, or lead, was electrified in the Voltaic circuit, the decomposition was very rapid, and an amalgam, or an alloy of potassium was obtained; the attraction between the common metals and the potassium apparently accelerating the separation of the oxigen.

the earths were mixed with similar oxides.

The idea that a similar kind of action might assist the decomposition of the alkaline earths induced me, to electrify mixtures of these bodies and the oxide of tin, of iron, of lead, of silver, and of mercury; and these operations were far more satisfactory than any of the others.

Barytes 2 p. oxide of silver 1 p.

A mixture of two thirds of barytes and one third of oxide of silver very slightly moistened was electrified by iron wires; an effervescence took place at both points of contact, and a minute quantity of a substance, possessing the whiteness of silver, formed at the negative point. When the iron wire to which this substance adhered was plunged into water containing a little alum in solution, gas was disengaged, which proved to be hidrogen; and white clouds, which were found to be sulphate of barytes, descended from the point of the wire.

Barytes and red oxide of mercury.

A mixture of barytes and red oxide of mercury, in the

and fifty of four inches, at this time was not more than equal to that of a newly constructed apparatus of one hundred and fifty of four inches. It had been made for the demonstrations in the Theatre of the Royal Institution in 1803; and since that time had been constantly employed in the annual courses of lectures, and had served, in different parts, for the numerous experiments on the decomposition of bodies by electricity, detailed in the Bakerian Lectures for 1806 and 1807, and a number of the plates were destroyed by corrosion. I mention these circumstances, because many chemists have been deterred from pursuing experiments on the decomposition of the alkalis and the earths, under the idea that a very powerful combination was required for the effect. This, however, is far from being the case; all the experiments detailed in the text may be repeated by means of a Voltaic battery, containing from one hundred to one hundred and fifty plates of four or six inches.

same

same proportions, was electrified in the same manner. A small mass of solid amalgam adhered to the negative wire, which evidently contained a substance, that produced barytes by exposure to air, with the absorption of oxigen; and which occasioned the evolution of hidrogen from water, leaving pure mercury, and producing a solution of barytes.

Lime, strontia, and magnesia with the same oxide.

Mixtures of lime, strontites, magnesia, and red oxide of mercury, treated in the same manner, gave similar amalgams, from which the alkaline earths were regenerated by the action of air or water, with like phenomena; but the quantities of metallic substances obtained were exceedingly minute; they appeared as mere superficial formations surrounding the point of the wire, nor did they increase after the first few minutes of electrization, even when the process was carried on for some hours.

A new battery employed.

These experiments were made previous to April, 1808, at which time the batteries were so much injured by constant use, as no longer to form an efficient combination. The inquiry was suspended for a short time: but in May I was enabled to resume it, by employing a new and much more powerful combination, constructed in the Laboratory of the Royal Institution, and consisting of five hundred pairs of double plates of six inches square.

When I attempted to obtain amalgams with this apparatus, the transmitting wires being of platina, of about $\frac{1}{40}$ of an inch in diameter; the heat generated was so great as to burn both the mercury and basis of the amalgam at the moment of its formation; and when, by extending the surfaces of the conductors, this power of ignition was modified, yet still the amalgam was only produced in thin films, and I could not obtain globules sufficiently large to submit to distillation. When the transmitting wires were of iron of the same thickness, the iron acquired the temperature of ignition, and combined with the bases of the earths in preference to the mercury, and metallic alloys of a dark gray colour were obtained, which acted on water with the evolution of hidrogen, and were converted into oxide of iron, and alkaline earths.

Pontin and Berzelius negatively electrified mercury in con-

While I was engaged in these experiments, in the beginning of June, I received a letter from Professor Berzelius

tact with barytes and lime.

zelius of Stockholm, in which he informed me, that, in conjunction with Dr. Pontin, he had succeeded in decomposing barytes and lime, by negatively electrifying mercury in contact with them, and that in this way he had obtained amalgams of the metals of these earths.

This repeated with success.

I immediately repated these operations with perfect success; a globule of mercury, electrified by the power of the battery of 500, weakly charged, was made to act upon a surface of slightly moistened barytes, fixed upon a plate of platina. The mercury gradually became less fluid, and after a few minutes was found covered with a white film of barytes; and when the amalgam was thrown into water, hidrogen was disengaged, the mercury remained free, and a solution of barytes was formed.

The result with lime, as these gentlemen had stated, was precisely analogous.

The same tried with strontia and magnesia.

That the same happy methods must succeed with strontites and magnesia, it was not easy to doubt, and I quickly tried the experiment.

From strontites I obtained a very rapid result; but from magnesia, in the first trials, no amalgam could be procured. By continuing the process however for a longer time, and keeping the earth continually moist, at last a combination of the basis with mercury was obtained, which slowly produced magnesia by absorption of oxigen from air, or by the action of water.

The amalgams might be preserved some time under naphtha.

All these amalgams I found might be preserved for a considerable period under naphtha. In a length of time, however, they became covered with a white crust under this fluid. When exposed to air, a very few minutes only were required for the oxigenation of the bases of the earths. In water the amalgam of barytes was most rapidly decomposed: that of strontites and that of lime next in order: but the amalgam from magnesia, as might be expected from the weak affinity of the earth for water, very slowly changed; when a little sulphuric acid was added to the water, however, the evolution of hidrogen, and the production and solution of magnesia were exceedingly rapid, and the mercury soon remained free.

Sulphate of a.

I was inclined to believe, that one reason why magnesia was

was less easy to metallize than the other alkaline earths was its insolubility in water, which would prevent it from being presented in the nascent state, detached from its solution at the negative surface. On this idea I tried the experiment, using moistened sulphate of magnesia, instead of the pure earth; and I found that the amalgam was much sooner obtained. Here the magnesia was attracted from the sulphuric acid, and probably deoxigenated and combined with the quicksilver at the same instant.

magnesia answered better than the earth.

The amalgams of the other bases of the alkaline earths could, I found, be obtained in the same manner from their saline compounds.

Salts of the other earths succeeded.

I tried in this way very successfully muriate and sulphate of lime, the muriate of strontites and of barytes, and nitrate of barytes. The earths, separated at the deoxigenating surface, there seemed instantly to undergo decomposition, and, seized upon by the mercury, were in some measure defended from the action of air, and from the contact of water, and preserved by their strong attraction for this metal.

III. *Attempts to procure the Metals of the alkaline Earths; and on their Properties.*

To procure quantities of amalgams sufficient for distillation, I combined the methods I had before employed, with those of Messrs Berzelius and Pontin.

Trial to procure the amalgams in larger quantities.

The earths were slightly moistened, and mixed with one third of red oxide of mercury; the mixture was placed on a plate of platina; a cavity was made in the upper part of it to receive a globule of mercury, of from 50 to 60 grains in the weight, the whole was covered by a film of naphtha, and the plate was made positive, and the mercury negative, by a proper communication with the battery of five hundred.

The amalgams obtained in this way were distilled in tubes of plate glass, or in some cases in tubes ef common glass. These tubes were bent in the middle, and the extremities were enlarged, and rendered globular by blowing, so as to serve the purposes of a retort and receiver.

The amalgams distilled.

The tube, after the amalgam had been introduced, was filled with naphtha, which was afterward expelled by

boiling,

boiling, through a small orifice in the end corresponding to the receiver, which was hermetically sealed when the tube contained nothing but the vapour of naphtha, and the amalgam.

Part of the mercury easily distilled off,

I found immediately, that the mercury rose pure by distillation from the amalgam, and it was very easy to separate a part of it; but to obtain a complete decomposition was very difficult.

but the whole only with great difficulty,

For this nearly a red heat was required, and at a red heat the bases of the earths instantly acted upon the glass, and became oxigenated. When the tube was large in proportion to the quantity of amalgam, the vapour of the naphtha furnished oxigen sufficient to destroy part of the bases: and when a small tube was employed, it was difficult to heat the part used as a retort sufficient to drive off the whole of the mercury from the basis, without raising too highly the temperature of the part serving for the receiver, so as to burst the tube*.

if at all.

In consequence of these difficulties, in a multitude of trials, I obtained only a very few successful results, and in no case could I be absolutely certain, that there was not a minute portion fmerc ury still in combination with the metals of the orths.

Base of barytes.

In the best result that I obtained from the distillation of the amalgam of barytes, the residuum appeared as a white metal of the colour of silver. It was fixed at all common temperatures, but became fluid at a heat below redness, and did not rise in vapour when heated to redness, in a tube of plate glass, but acted violently upon the glass, producing a black mass, which seemed to contain barytes, and a fixed alkaline basis, in the first degree of oxigenation †.

When

* When the quantity of the amalgam was about fifty or sixty grains, I found that the tube could not be conveniently less than one sixth of an inch in diameter, and of the capacity of about half a cubic inch.

Bases of the earths probably the most powerful means of detecting oxigen.

† From this fact, compared with other facts that have been stated, p. 369, it may be conjectured, that the basis of barytes has a higher affinity for oxigen than sodium; and hence probably the bases of the earths will be more powerful instruments for detecting oxigen, than the bases of the alkalis.

I have

When exposed to air, it rapidly tarnished, and fell into a white powder, which was barytes. When this process was conducted in a small portion of air, the oxigen was found absorbed;

I have tried a number of experiments on the action of potassium on bodies supposed simple and on the undecompounded acids. From the affinity of the metal for oxigen, and of the acid for the substance formed, I had entertained the greatest hopes of success. It would be inconsistent with the object of this paper to enter into a full detail of the methods of operation; I hope to be able to state them fully to the Society at a future time, when they shall be elucidated by farther researches; I shall now merely mention the general results, to show that I have not been tardy in employing the means which were in my power, towards effecting these important objects.

Base of potash applied to

When potassium was heated in muriatic acid gas, as dry as it could be obtained by common chemical means, there was a violent chemical action with ignition; and when the potassium was in sufficient quantity, the muriatic acid gas wholly disappeared, and from one third to one fourth of its volume of hidrogen was evolved, and muriate of potash was formed.

muriatic acid gas,

On fluoric acid gas, which had been in contact with glass, potassium produced a similar effect; but the quantity of hidrogen generated was only one sixth or one seventh of the volume of gas, and a white mass was formed, which principally consisted of fluate of potash and silex, but which emitted fumes of fluoric acid when exposed to air.

fluoric acid gas,

When boracic acid, prepared in the usual manner, that had been ignited, was heated in a gold tube with potassium, a very minute quantity of gas only was liberated, which was hidrogen, mixed with nitrogen, (the last probably from the common air in the tube); borate of potash was formed, and a black substance, which became white by exposure to air.

and boracic acid.

In all these instances there is great reason to believe that the hidrogen was produced from the water adhering to the acids; and the different proportions of it in the different cases are a strong proof of this opinion. Admitting this idea, it seems, that muriatic acid gas must contain at least one eighth or one tenth of its weight of water; and that the water oxigenates in the experiment a quantity of potassium, sufficient to absorb the whole of the acid.

The results not conclusive.

In the cases of fluoric and boracic acids, there is probably a decomposition of these bodies; the black substance produced from the boracic acid is similar to that which I had obtained from it by electricity. The quantities that I have operated upon have been as

yet

absorbed, and the nitrogen unaltered; when a portion of it was introduced into water, it acted upon it with great violence and sunk to the bottom, producing in it barytes; and hidrogen was generated. The quantities in which I obtained it were too minute for me to be able to examine correctly either its physical or chemical properties. It sunk rapidly in water, and even in sulphuric acid, though surrounded by globules of hidrogen, equal to two or three times its volume; from which it seems probable, that it cannot be less than four or five times as heavy as water. It flattened by pressure, but required a considerable force for this effect.

Base of strontia.

The metal from strontites sunk in sulphuric acid, and exhibited the same characters as that from barytes, except in producing strontites by oxidation.

Base of lime.

The metal from lime I have never been able to examine exposed to air or under naphtha. In the case in which I was able to distil the quicksilver from it to the greatest extent, the tube unfortunately broke, while warm, and at the moment that the air entered, the metal, which had the colour and lustre of silver, instantly took fire, and burnt with an intense white light into quicklime.

Base of magnesia.

The metal from magnesia seemed to act upon the glass, even before the whole of the quicksilver was distilled from it. In an experiment in which I stopped the process before the mercury was entirely driven off, it appeared as a solid, having the same whiteness and lustre as the other metals of

yet too small, to enable me to separate and examine the products, and till this is done, no ultimate conclusion can be drawn.

The action of potassium upon muriatic acid gas indicates a much larger quantity of water in this substance, than the action of electricity in Dr. Henry's elaborate experiments; but in the one instance the acid enters into a solid salt, and in the other it remains aeriform; and the difficulty of decomposition by electricity must increase in proportion as the quantity of water diminishes, so that at the apparent maximum of electrical effect, there is no reason to suppose the gas free from water.

Those persons who have supposed hidrogen to be the basis of muriatic acid may, perhaps, give another solution of the phenomena, and consider the experiment I have detailed as a proof of this opinion.

the

the earths. It sunk rapidly in water, though surrounded by globules of gas producing magnesia, and quickly changed in air, becoming covered with a white crust, and falling into a fine powder, which proved to be magnesia.

In several cases in which amalgams of the metals of the earths, containing only a small quantity of mercury, were obtained, I exposed them to air on a delicate balance, and always found, that, during the conversion of metal into earth, there was a considerable increase of weight. Amalgams of the metals of the earths.

I endeavoured to ascertain the proportions of oxigen and basis in barytes and strontites, by heating amalgams of them in tubes filled with oxigen, but without success. I satisfied myself, however, that when the metals of the earths were burned in a small quantity of air they absorbed oxigen, gained weight in the process, and were in the highly caustic or unslacked state; for they produced strong heat by the contact of water, and did not effervesce during their solution in acids. Attempts to ascertain the proportion of base.

The evidence for the compostion of the alkaline earths is then of the same kind as that for the composition of the common metallic oxides; and the principles of their decomposition are precisely similar, the inflammable matters in all cases separating at the negative surface in the Voltaic circuit, and the oxigen at the positive surface.

These new substances will demand names; and on the same principles as I have named the bases of the fixed alkalis potassium and sodium, I shall venture to denominate the metals from the alkaline earths barium, strontium, calcium, and magnium; the last of these words is undoubtedly objectionable, but magnesium * has been already applied to metallic manganese, and would consequently have been an equivocal term. New names.

IV. *Inquiries relative to the Decomposition of Alumine, Silex, Zircone, and Glucine.*

I tried the methods of electrization and combination with quicksilver, and the common metals, by which I had succeeded in decomposing the alkaline earths, on alumine and Alumine and silex tried as the other earths.

* Bergman. Opusc. tom. ii, p. 200.

silex;

silex; but without gaining distinct evidences of their having undergone any change in the processes.*

Obliged to seek for other means of acting upon them, it was necessary to consider minutely their relations to other bodies, and to search for analogies, by which the principles of research might be guided.

Nearly indifferent to the two electricities.

Alumine very slowly finds its point of rest at the negative pole, in the electrical circuit; but silex, even when diffused in its gelatinous state through water, rests indifferently at the negative or positive poles.

Analogous to insoluble neutral salts, or saturated oxides.

From this indifference to positive and negative elecrical attractions, following the general order of facts, it might be inferred, that if these bodies be compounds, the electrical energies of their elements are nearly in equilibrium; and that their state is either analogous to that of insoluble neutral salts, or of oxides nearly saturated with oxigen.

The combinations of silex and alumine with acids and alkalis, as well as their electrical powers, were not inconsistent with either of these ideas; for in some respects they resemble in physical characters fluate and phosphate of lime, as much as in others they approach to the oxides of zinc and tin.

Experiments to resolve silex, if neutrosaline.

On the idea that silex might be an insoluble neutrosaline compound, containing an unknown acid or earth, or both, and capable of being resolved into its secondary elements, in the same manner as sulphate of barytes, or fluate of lime, I made the following experiments.

Exposed to electricity in water.

Two gold cones*, connected by moistened amianthus, were filled with pure water, and placed in the electrical circuit, a small quantity of carefully prepared and well washed silex was introduced into the positive cone; the action was kept up from a battery of two hundred plates, for some hours, till nearly half of the fluid in each cone was exhausted; the remainders were examined; the fluid in the cone containing the silex was strongly acid: that in the opposite cone was strongly alkaline; the two fluids were passed through bibulous paper, and mixed together, when a precipitate fell down, which proved to be silex.

One vessel acid, the other alkaline, and silex dissolved.

* The same as those described in Phil. Trans. 1807, p. 6; or Journal, vol. xviii, p. 325.

On

On the first view of the subject, it appeared probable, that this silex had been formed by the union of the acid and the alkaline matter in the two cones, and that the experiment demonstrated a decomposition and recomposition of silex; but before such a conclusion could be made, many points were to be determined.

It might be supposed, that the silex was decomposed, and then recomposed,

It was possible, that the acid might be nitric acid, produced as in other electrical experiments of a similar nature, and that this acid might have dissolved silex, which was precipitated by the alkaline matter at the other pole, which might be either potash used for dissolving the silex, which had adhered to it, notwithstanding the processes of lixiviation in acids, or ammonia produced in consequence of the presence of the atmosphere; or if potash was present, it was likewise possible, that the silex might have been carried over in solution, with this alkali, from the positive to the negative surface.

Minute experiments were instituted and completed in the same manner as those detailed in the Philosophical Transactions for 1807, p. 7 *, which soon proved, that there was no reason to suppose, that the silex had been changed in these experiments.

but this not the fact.

The acid proved to be nitric acid, which under the electrical action seemed to have dissolved the silex; the alkali turned out to be principally fixed alkali; and that it was merely an accidental ingredient, and not a constituent of the silex, appeared from this circumstance, that when the same portion of silex was long electrified, by degrees it lost its power of affording the substance in question †.

The acid was nitric.

Alkali not from the silex

This

* Journal, vol. xviii, p. 325.

† If silex, that has been carefully washed, after precipitation by muriatic acid from liquor silicum, be moistened, and acted on by mercury negatively electrified, the mercury soon contains a notable quantity of potassium. Well washed alumine, that has been precipitated from alum by carbonate of soda, affords by the same treatment sodium and potassium, so that the powers of electrochemical analysis are continually demonstrating the imperfection of the common chemical methods of separating bodies from each other. The purest boracic acid, which can be obtained from borax by chemical decomposition, by electrical analysis is shown to contain both soda, and the decomposing acid employed in the process; and hence

Common chemical methods of separating bodies imperfect.

Treated as inflammables saturated with oxigen.

This result having taken place, the same plan of operation was not pursued with respect to alumine, which resembles a saline compound less than silex; and the method which I now adopted of acting upon these bodies was on the supposition of their being inflammable substances so highly saturated with oxigen as to possess little or no positive electricity.

Alumine and silex have both a strong affinity for potash and soda; now supposing them to be oxides, it was reasonable to conclude, that the oxigen, both in the alkalis and the earths, must be passive as to this power, which must consequently be referred to their bases, and on this notion it was possible, that it might be made to assist their decomposition by electricity.

Silex 1 p. potash 6 p. kept in fusion in a platina crucible and electrified.

After this reasoning, I fused a mixture of one part of silex, and six of potash in a platina crucible, and preserved the mixture fluid, and in ignition, over a fire of charcoal; the crucible was rendered positive from the battery of five hundred, and a rod of platina, rendered negative, was brought into contact with the alkaline menstruum. At the moment of contact there was a most intense light; when the rod was plunged into the liquid an effervescence took place, and globules, which burnt with a brilliant flame, rose to the surface, and swam upon it in a state of combustion. In a few minutes, when the mixture was cool, the platina bar was removed: after as much as possible of the alkali and silex had been detached from it by a knife, there remained brilliant metallic scales round it, which instantly became covered with a white crust in the air, and some of which inflamed spontaneously. The platina appeared much corroded, and of a darker tint than belongs to the pure metal. When it was plunged into water it strongly effervesced: the fluid that came from it was alkaline; when a few drops of muriatic acid were added to the solution, a white cloudiness occurred, which various trials demonstrated to depend upon the presence of silex.

hence the experiment on the action of the boracic acid and potassium, page 375, may possibly be explained without assuming its decomposition.

A similar

A similar mixture of potash and alumine was experimented upon in the same manner, and the results were perfectly analogous; there adhered to the rod of platina a film of a metallic substance, which rapidly decomposed water, and afforded a solution which deposited alumine by the action of an acid.

Alumine and potash treated in the same manner.

I tried several forms of this experiment, with the hopes of being able to obtain a sufficient quantity of the metallic matter from the platina, so as to examine it in a separate state; but I was not successful. It was always in superficial scales, which oxidated, becoming white and alkaline, before it could be detached in the air; it instantly burnt when heated, and could not be fused under naphtha or oil.

The metal could not be obtained separate.

I tried similar experiments with mixtures of soda and alumine, and soda and zircone, and used iron as the negatively electrified metal. In all these cases, during the whole process of electrization, abundance of globules, which swam in a state of inflammation on the fused mass, were produced. And in the mixture, when cooled, small laminæ of metal were found of the colour of lead, and less fusible than sodium, which adhered to the iron; they acted violently upon water, and produced soda and a white powder, but in quantities too small to be minutely examined.

Experiments with alumine and soda, and zircone and soda.

I endeavoured to procure an alloy of potassium, and the bases of the earths, from mixtures of potash, silex, and alumine, fused by electricity, and acted on by the positive and negative surfaces in the same manner as pure potash, in experiments for the decomposition of that substance; but I obtained no good results. When the earths were in quantities equal to one fourth or one fifth of the alkali, they rendered it so highly nonconducting, that it was not easy to affect it by electricity, and when they were in very minute portions, the substance produced had the characters of pure potassium.

Trials to obtain an alloy by treating as potash unsuccessful.

I heated small globules of potassium, in contact with silex and alumine, in tubes of plate glass filled with the vapour of naphtha: the potassium seemed to act at the same time upon the glass and the earths, and a grayish opaque mass, not possessed of metallic splendour, was obtained, which effervesced in water, depositing white clouds. Here it was

Potassium heated with silex and alumine in vapour of naphtha.

Inconclusive.

possible that the potash had been converted wholly or partly into protoxide, by its action upon the earths; but as no globule was obtained, and as the plate glass alone might have produced the effect, no decided inference of the decomposition of the earths can be drawn from the process.

I shall now mention the last trials that I made with respect to this object.

Amalgam of potassium electrified with silex,

Potassium, amalgamated with about one third of mercury, was electrified negatively under naphtha, in contact with silex very slightly moistened, by the power of five hundred; after an hour the result was examined. The potassium was made to decompose water, and the alkali formed neutralized by acetous acid; a white matter, having all the appearance of silex precipitated, but in quantity too small for accurate examination.

alumine, glucine,

I tried the same method of action upon alumine and glucine, and obtained a cloudiness, more distinct than in the case of silex, by the action of an acid upon the solution obtained from the amalgam.

and zircone.

Zircone exposed in the same manner to the action of electricity, and the attraction of potassium, furnished still more satisfactory results, for a white and fine powder, soluble in sulphuric acid, and which was precipitated from sulphuric acid by ammonia, separated from the amalgam that had been obtained by the action of water.

They all appear to be metallic oxides,

From the general tenor of these results, and the comparison between the different series of experiments, there seems very great reason to conclude that alumine, zircone, glucine, and silex are, like the alkaline earths, metallic oxides, for on no other supposition is it easy to explain the phenomena, that have been detailed.

but the evidence not so strict.

The evidences of decomposition and composition are not however of the same strict nature as those that belong to the fixed alkalis and alkaline earths; for it is possible, that in the experiments in which the silex, alumine, and zircone appeared to separate during the oxidation of potassium and sodium, their bases might not actually have been in combination with them, but the earths themselves in union with the metals of the alkalis, or in mere mechanical mixture. And out of an immense number of experiments, which I made

made of the kind last detailed, a very few only gave distinct indications of the production of any earthy matter; and in cases when earthy matter did appear, the quantity was such, as rendered it impossible to decide on the species.

Had I been so fortunate as to have obtained more certain evidences on this subject, and to have procured the metallic substances I was in search of, I should have proposed for them the names of silicium, alumium, zirconium, and glucium.

(To be concluded in our next.)

SCIENTIFIC NEWS.

Wernerian Natural History Society.

AT the meeting of the Wernerian Natural History Society on the 12th of November, the Rev. Andrew Jameson, minister of St. Mungo, Dumfriesshire, read a paper entitled Observations on Meteorological Tables, with a description of a new Anemometer. After some general observations on the importance of meteorological observations, and on the merits and defects of registers of the weather, &c., he pointed out what he considered to be the best form of a meteorological journal, and then described the external form and internal structure of an extensive and complete meteorological observatory, and enumerated about twenty different instruments, which ought to find a place in every establishment of that kind. He remarked, that a daily examination of the changes which take place in those instruments, joined with a careful record of the external appearances in the atmosphere, will afford a constant and fascinating employment to the most zealous observer, and will in time enable us to form a just theory of meteors; to prognosticate with considerable accuracy the nature of the coming weather; and, lastly, enable us to ascertain the climate of different countries, with the view of determining the influence it exerts on organic bodies. He next described an Anemometer, which, by a very simple and ingenious arrangement of parts, will enable the most common observer to ascertain the velocity of the wind with perfect accuracy. Wernerian Society. Meteorological observations. Their use.

At the same meeting, the Rev. John Fleming, F. A. S. minister of Bressay in Shetland, who has been for some time Mineralogy of the Shetland isles.

Mineralogy of the Shetland isles.

time past employed in examining the mineralogy of those remote islands, communicated to the Society an interesting account of the geognostic relations of the rocks in the islands of Unst and Papa Stour; in the course of which he gave answers to the queries formerly published regarding the serpentine and sandstone of Shetland. After a general account of the position, extent and external appearance of the island of Unst, he next described the different rocks of which it is composed, in the order of their relative antiquity, and remarked, that their general direction is from S. W. to N. E. The rocks are gneiss, mica-slate, clay-slate, limestone, hornblende-rock, potstone, and serpentine.—The *gneiss* in some places appeared to alternate with the oldest mica-slate, and in others to contain beds of hornblende-rock. The *mica-slate*, which is the most abundant rock in the island, is traversed by numerous contemporaneous veins of quartz, and also of feldspar, and passes distinctly into clay-slate. It contains beds of hornblende-rock and of limestone. The *clay-slate* occurs but sparingly in this island. The *potstone* usually accompanies the serpentine. The *serpentine* occurs in great abundance, *in beds*, in the oldest clay-slate and newest mica-slate, and hence must be referred to the oldest or first serpentine formation of Werner. Mr. Fleming is also inclined to believe, that the serpentine of the neighbouring island of Fetlar belongs to the same formation. The island of Papa Stour, situate on the west coast of the Mainland, (as the largest of the islands is called), contains no primitive rocks; on the contrary, it appears to be entirely composed of floetz rocks. These are, conglomerate, greenstone, claystone, porphyritic stone, hornstone, and sandstone. The sandstone, as appears from observations made in this island and other parts of Shetland, would seem to belong to the oldest coal-formation. The claystone, conglomerate, porphyritic stone, greenstone, and hornstone (probably clinkstone) rest on the sandstone. In some places Mr. Fleming observed the greenstone alternating with the sandstone, hence he properly concludes, that they belong to the same formation. In no place, however, did he observe any of the other rocks alternating with the sandstone; and therefore the formation to which they belong remains still somewhat problematical.

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